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MERISM AND SEX IN "SPINAX NIGER."

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I. INTRODUCTION.

IN spite of the mass of literature steadily accumulating upon the subject, the question of the origin of limbs still remains one of the most vexed problems in vertebrate morphology. The gill-arch theory of Gegenbaur and the side-fold theory with which the name of Balfour is most closely associated each have their supporters. Both theories are so elastic that each without undue stretching may be made to cover all the facts at present known with regard to the comparative

anatomy, embryology, and palaeontology of the limbs themselves. There is however one fundamental difference between them. On the gill-arch theory the various positions of the limbs in the meristic series involve the conception of limb-migration or homoeosis. On the side-fold theory these positions may be supposed to be due to processes of excalation or intercalation of segments, though the possibility of homoeosis is not necessarily excluded. Its inclusion however cannot but detract from the simplicity of the theory. Consequently, if on independent grounds the phenomenon of homoeosis is found to be non-existent, the presumption in favour of the side-fold theory is very great. If, on the other hand, this phenomenon can be shown to be a reality, the gill-arch theory must gain considerable support. For with the balance of evidence equally poised, Gegenbaur's theory has the merit of deducing the limbs from a structure already in existence. In the present paper a considerable number of one of the most primitive of living vertebrates has been examined and the meristic variations studied with the aid of biometric methods. The key to the limb question has been sought in the vertebral column, and the evidence there gained in favour of homoeosis lends support to the gill-arch as opposed to the side-fold theory of the origin of the vertebrate limb.

Before passing on to a consideration of the evidence I would here express my sense of obligation to the Government Grant Committee of the Royal Society for opportunities of visiting Norway for material; to Dr Nordgaard, Director of the Marine Biological Station at Bergen, for his most kind assistance in procuring that material; and to Professor Karl Pearson for much generous help in connection with the working out of the statistics.

Material and Methods.

The material brought together consists in all of 567 specimens of *Spinax niger*, of which 263 are adult (100 ♂s and 163 ♀s), whilst the remaining 304 are embryos almost ready to be born (145 ♂s and 159 ♀s). In the adults of both sexes the following points were noted in connection with the vertebrae, spines, and pelvic plexus, though, owing to technical difficulties consequent upon the small size of the embryos, the condition of the pelvic plexus in these was not examined. The raw data are given in the Appendix, Tables 11 to 47.

1. *Whole vertebrae.* As in all Elasmobranchs the vertebral column is composed of whole vertebrae in the trunk region and of half vertebrae in the caudal region. The whole vertebrae are therefore delimited rostrally by the skull and caudally by the half vertebrae. The junction of the whole vertebrae with the skull is rendered fairly simple of determination by the condition of the spino-occipital nerves. When the skull is viewed from the inside a small foramen is seen just below that for the vagus (Plate I, Figs. 4 and 5). This foramen, which is constant in its position, I regard as belonging to the most anterior of the spino-occipital nerves (x). Neither this nerve nor y immediately behind it ever possesses

a dorsal root in the adult. A dorsal root may be exhibited by the last spino-occipital nerve, though in most cases it is lacking. The arrangement of the cartilages in this region is somewhat irregular. When the dorsal root of z is present the hinder part of the skull is longer than usual, and, whilst z^v always passes through the skull, z^d issues through a separate intercalary cartilage. In other words, where there is least condensation in this region of the skull, the basidorsal cartilage corresponding with nerve z is fused with the cranium, whilst the interdorsal remains separate and transmits z^d if this be present. When z^d is absent the interdorsal may remain as a separate cartilage, or it may fuse with the basidorsal next behind it. An idea of the amount of variation in this region may be obtained from Figs. 4 and 5 on Plate I. The existence of such variations serves to greatly strengthen the theory of vertebral condensation, though the point with which we are here concerned is that they are not of sufficient magnitude to materially confuse the nomenclature of this region. The anterior limit of the whole vertebrae is capable of rigid demarcation.

The junction of the whole and half vertebrae is in most cases easily determined, and the two most rostral of the half vertebrae each show a basidorsal and an interdorsal cartilage on either side. Over the 1st, 3rd, 5th, ... half vertebrae there are no nerve foramina; they occur only over the even-numbered half vertebrae. From this normal condition variations of two sorts occur. There may be only two pairs of cartilages over the first two half vertebral centra taken together (Fig. 6 *a*, Plate I), a very large basidorsal and a small interdorsal, which latter, as usual, affords a passage for the dorsal root. In fact, we have here a whole vertebra in which the centrum has become divided, without division of the cartilages forming the neural arch. For purposes of this paper, such a condition has to be reckoned as two half vertebrae. The second form of variation is an incomplete division of the centrum of the last whole vertebra. The division may be incipient only, or it may be fairly well marked on one side whilst absent on the other. In such cases, which are comparatively rare, the centrum exhibiting such incomplete division has been reckoned as that of the last whole vertebra.

2. *Half vertebrae.* The rostral limit of the half vertebrae has already been discussed. The caudal limit in *Spinax* is quite well defined, and little difficulty was experienced in deciding the number when the cleaned skeleton of the tail-fin was examined with a simple lens, though the higher powers of a dissecting microscope were used in the case of the embryos. It has been assumed that all the vertebrae, from the junction with the whole vertebrae to the tip of the tail, fall into the same category, viz. that of half vertebrae.

Ridewood ('**99**, p. 55) has recently disputed this on the ground that towards the caudal end the myotomes are twice as numerous as in the rostral part of the series. As this point is of great importance for the present paper I may here give the reasons which have led me to dissent from the view taken by Ridewood.

(*a*) On the hypothesis that the half vertebrae do not extend to the end of the tail, but that at some point the whole vertebral condition is resumed, one would naturally look for a transitional portion such as occurs more rostrally between the whole and half vertebrae. No such portion is however to be found. The centra of the half vertebrae become gradually and uniformly smaller till they cease.

(*β*) Occasional variations are to be met with in which an unusually long centrum occurs among the half vertebrae (cf. Plate I, Fig. 3). Such a variation is easily explicable on the assumption that it is a whole vertebra which has failed to split into two half vertebrae. Such centra I have reckoned as equivalent to two half vertebrae.

(*γ*) Ridewood states that in *Acanthias vulgaris* the transition from half vertebrae back to whole vertebrae occurs at about the 24th vertebra from the caudal extremity. This statement led me to examine the position of the spinal nerves in three specimens of this species. The last few segments have no spinal nerves issuing out from them but the attenuated myotomes in this region receive their innervation from nerves passing out more rostrally. By opening the neural canal and using osmic acid the minutest caudal roots were evident. Their mode of exit was uniform to their end, i.e. over every even-numbered half vertebral centrum. Counting gave the following results:

	Total number of half vertebrae	Last half vertebra over which a nerve root passes out
Specimen A ...	67	46
" B ...	63	50
" C ...	65	48

In these three cases there could not have been more than 21, 13, and 17 whole vertebrae respectively caudal to the half vertebrae, whilst from the arrangement of the myotomes Ridewood claims that there are 24. Taking all the evidence together, and admitting that the myotomes may show some difference of attachment at the end of the tail, there can, I think, be very little doubt but that the centra throughout the caudal region belong to the same class, viz. that of half vertebrae.

3. *Total segments.* The number of these was obtained by adding the number of the whole vertebrae to the number of the half vertebrae divided by two. Odd half vertebrae were counted as a whole segment, e.g. a specimen with 44 whole vertebrae and 41 half vertebrae was reckoned as possessing $44 + 21 = 65$ segments.

4. *Anterior spine.* The position of this was determined by a sagittal section. It articulates most commonly with the basidorsal and interdorsal cartilages of a single vertebra (Plate I, Fig. 1 *b*). In such cases it has been reckoned as belonging

to that vertebra. Sometimes it articulates with the interdorsal of vertebra x and the basidorsal of $x+1$ (Plate I, Fig. 1 *c*). In such cases it has been reckoned as belonging to segment x. Lastly, it may be carried mainly by a basidorsal, though to a slight extent by the interdorsals in front and behind (Plate I, Fig. 1 *a*). Here it has been regarded as pertaining to the segment to which the basidorsal belongs.

5. *Posterior spine.* A uniform system of nomenclature has been adopted throughout, and what applies to the anterior applies also to the posterior spine.

6. *1st girdle-piercing nerve* (*1st g. p. n.*). The pelvic girdle is pierced by one or two foramina transmitting certain of the ventral divisions of the fin-nerves. When only one foramen is present it transmits the ventral division of the nervus collector. When, as is less frequently the case, two foramina are found, the anterior of them transmits the collector. The most caudal of the nerves which go to form the collector therefore passes down almost directly to the foramen, and it is this, the 1st girdle-piercing nerve, that I have used to estimate the position of the pelvic girdle with reference to the axial skeleton.

7. *The nervus collector* contains in *Spinax* several branches lying along the lateral vein (cf. Braus, '**98**, Fig. 5, Plate XI). By injecting this vessel with osmic acid, in the way that I have previously described for *Mustelus* ('**00**, p. 332), the smallest branches can be readily made out.

8. As *post-girdle nerves* (*p. g. n.*), I designate all such as run to the fin on the caudal side of the 1st g. p. n. Thus, if there are two foramina in the pelvic girdle, the nerve passing through the hinder one will be the 1st post-girdle nerve. It not infrequently happens that the fin branch of the most caudal fin nerve joins with the fin branch of the most caudal but one before entering the fin, thus forming a "posterior collector"; more rarely the "posterior collector" may be composed of branches from three nerves. Such a condition is brought about by persistence of the early embryonic state of things in which a posterior collector is normally present, as Braus has already shown ('**02**, p. 566).

9. The *fin rays* were counted for the pelvic fins of a number of ♀s. A slight difficulty was introduced here by the fact that the last ray may be more or less fused with the distal part of the metapterygium. Whether it was more or less fused, or quite free, the last ray was always reckoned independently.

10. The *length of the whole and half vertebral series* was determined in a number of ♂s. The measurements were made from centrum to centrum.

The observations made upon the various above-mentioned characters were tabulated separately for adults and embryos, and again for each sex apart. For each of the series so obtained the mean and standard deviation were calculated together with the probable error in each instance. The results are given in full in the Appendix, Tables 12—19.

Sexual Dimorphism.

One of the most striking points brought out by these figures is a well-marked sexual dimorphism permeating all the meristic features investigated. This is well shown in Table 1, below, and equally for the embryos as for the adults. On looking down this table it will be noticed that the ♀ adults differ from the

TABLE 1.

Comparison of the mean (M.) *and its probable error* (P. E. M.) *between the sexes and between embryos and adults of each sex.*

	♂ adults (100)	♂ embryos (145)	♀ adults (163)	♀ embryos (159)
Anterior spine ... ... M.	15·979	15·965	16·159	16·056
P. E. M.	± ·046	± ·031	± ·040	± ·031
Posterior spine ... ... M.	40·540	40·476	41·170	40·686
P. E. M.	± ·060	± ·051	± ·048	± ·040
Whole vertebrae... ... M.	44·620	44·510	45·061	44·723
P. E. M.	± ·077	± ·074	± ·057	± ·058
Half vertebrae ... ... M.	40·370	40·034	40·288	39·956
P. E. M.	± ·128	± ·107	± ·101	± ·091
Total segments ... ... M.	65·040	64·724	65·472	64·906
P. E. M.	± ·071	± ·065	± ·055	± ·056
First girdle-piercing nerve M.	28·470	—	29·057	—
P. E. M.	± ·041		± ·030	
Post-girdle nerves ... M.	9·360	—	7·920	—
P. E. M.	± ·039		± ·030	
Collector nerves ... ... M.	5·230	—	5·304	—
P. E. M.	± ·030		± ·024	

The question whether any two values of a statistical constant have a significant difference in value is to be settled by comparing that difference with the probable error of that difference, which by a well-known theorem in the theory of errors is the square root of the sum of the squares of the probable errors of the two given values. Professor Pearson to whom I am indebted for this statement considers odds less than those corresponding to twice the probable error as not definitely significant, with odds corresponding to twice up to thrice the probable error we have probable significance, and with more than three times the probable error there is almost certain significance. Of course a difference less than twice the probable error does not *prove* that the difference is insignificant, it may merely indicate that the statistics are insufficient in number to adequately distinguish significant differences. Again, persistent differences of the same sign, when each difference is even less than the probable error, increase the odds in favour of a general significance.

♂s in possessing a greater number of whole vertebrae and total segments, as well as in the fact that both the anterior and posterior spines are more caudally situated. In all these cases, except that of the half vertebrae and collector nerves, the difference in the means of the two sexes is more than three times the probable

error of the difference, and is therefore doubtless significant. A comparison of these figures for the embryos reveals a similar condition of things with this small difference, viz. that the means of the two sexes are not quite so widely separated as in the adults. See Table 1 *bis*, p. 320. With regard to half vertebrae the ♂ embryos tend to show a rather larger number than the ♀s. The difference however is not so marked as in the features just considered, and falls within the limit of the probable error. Consequently, whilst it would be hazardous to attach much weight to this difference, yet the fact that it is found equally well marked in both embryos and adults lends some support to the view that it may not be altogether without significance.

In the cases of the 1st g. p. nerve and the post-girdle nerves, the differences in the means for the two sexes are so great that there can be no doubt but that we are here dealing with characters exhibiting a marked sexual dimorphism. The difference in the number of collector nerves is also nearly double the probable error of the difference and is therefore possibly significant.

The fact that for both cases the mean of the embryos is in all cases less than that of the adults, coupled with the fact that these differences are probably in every case significant, would seem to denote one of two things. Either selection has been at work during post-embryonic existence, and in such a way that the factors concerned have operated in the same direction for both sexes, or else an increase in the number of segments takes place during this period. But these are points to which reference will be made later.

The figures given in Table 1 show that a well-marked sexual dimorphism occurs, and that it cannot be due to selection, since it is almost equally apparent among the unborn young. Such sexual differences have been shown to exist also among other Elasmobranchs where considerable numbers have been examined, viz. in *Mustelus laevis* ('**00**, p. 339), and in *Acanthias vulgaris* ('**01**, p. 24). From which it follows that in attempting to trace ontogenetic changes from a numerical standpoint, this factor must be taken into account as well as the variability of these structures; and any attempt that neglects these factors must necessarily be of comparatively little value. It seems a plausible view that the sexual dimorphism which occurs in the Elasmobranchs is due primarily to the presence of the claspers which are found in the ♂. The enlargement of the distal part of the pelvic fin necessitated by the development of these structures has led to a more rostral position of the pelvic girdle, which in its turn involves correlated changes throughout the meristic series. If there be any truth in the view that the claspers are the determining cause of the meristic differences in the sexes, one would be led to expect no such difference among the Teleostei, where such organs are absent. The only member of that group which, so far as I am aware, has been examined in this connection is *Clupea harengus*. In his elaborate study of that fish, Heincke ('**98**, p. 95) has shown that there are no sexual differences with regard to the vertebrae or to the position of the fins, paired and unpaired.

*TABLE 1 *bis.*

Significance in Difference of Type.

Δ_{st} = difference of means of classes s and t.

$R = \Delta_{st} / \sqrt{\text{sum of squares of probable errors of means.}}$

Test of significance as on p. 318.

Character	♂ adult—♀ adult			♂ embryo—♀ embryo			♂ embryo—♂ adult			♀ embryo—♀ adult		
	Δ	R	Significance	Δ	R	Significance	Δ	R	Significance	Δ	R	Significance
Anterior spine	− ·180	− 3·0	Highly probable	− ·091	−2·1	Probable	− ·014	− ·3	None	− ·103	− 2·0	Probable
Posterior spine	− ·630	− 8·2	Highly probable	− ·210	− 3·2	Highly probable	− ·064	− ·8	None	− ·484	− 7·7	Highly probable
Whole vertebrae ...	− ·441	− 4·6	Highly probable	− ·213	− 2·3	Probable	− ·110	− 1·0	None	− ·338	− 4·2	Highly probable
Half vertebrae	+ ·082	+ ·5	None	+ ·078	+ ·6	None	− ·336	− 2·0	Probable	− ·332	− 2·4	Probable
Total segments	− ·432	− 4·8	Highly probable	− ·182	− 2·1	Probable	− ·324	− 3·4	Highly probable	− ·566	− 7·2	Highly probable
1st girdle-piercing nerve	− ·587	− 11·6	Highly probable	—	—	—	—	—	—	—	—	—
Post-girdle nerves ...	+ 1·440	+ 29·3	Highly probable	—	—	—	—	—	—	—	—	—
Collector nerves ...	− ·074	− 1·9	Doubtful	—	—	—	—	—	—	—	—	—

It will be found that while the ♂ and ♀ differences are highly significant for both adults and embryos in all but one case, the differences between embryos and adults are more markedly significant for ♀s than ♂s. In the latter case, however, the signs are, as in the former, *all alike*, thus increasing the odds in favour of significance from the combined cases. The weighted mothers, while significantly different from the adult ♀s as far as variation is concerned (see p. 325) appear to be only significantly different in type for the case of half vertebrae.

* [For this Table and for Tables 3 and 5 *c* I am indebted to Professor Pearson. R. C. P.]

The Numerical Proportions of the Sexes.

In connection with what has gone before a brief digression on the proportional numbers of the sexes may not be out of place. Storm's ('80) experience was that the ♂s of *Spinax* were rarer than the ♀s, though he gives no numbers. Records of the numbers of each sex caught off Ask have been made at different times by Braus, Nordgaard, and myself, and are given in the subjoined table.

	♂	♀
*Braus, July, 1897	121	98
Punnett, July, 1901	37	83
„ June-July, 1902 ...	47	65
Nordgaard, July, 1903	13	23
	218	269

The collected figures agree with Storm's conclusion, the only case where the ♂s were more numerous being Braus' experience in 1897. Braus also collected at Drobak, near Christiania, where he found that the ♂s were very much scarcer than the ♀s, being in fact only half as numerous. There are good grounds therefore for supposing that in the adult state the ♂s are less numerous than the ♀s. The sexes are, however, produced in practically equal numbers, for out of a total of 308† embryos were found 149 ♂s and 159 ♀s. And this conclusion is strengthened by the fact that in the three different years, 1901–3, in which embryos were collected, the proportion of ♂s to ♀s was respectively 35 : 39, 48 : 46, and 66 : 74. Consequently from a consideration of the proportional numbers of the sexes before birth and during adult life we are led to conclude that post-natal mortality is greater among the ♂s than among the ♀s. This conclusion is of considerable importance, for, as will appear below, statistical treatment of the meristic variations points independently in the same direction.

II. VARIATION AND CORRELATION.

Meristic Variability.

The measure of the variability used is the standard deviation (σ) which has been calculated separately for the adults and embryos, and for both sexes. The results are given in Table 2, and allow of a comparison between the sexes, for embryos as well as for adults. As there is some irregularity in this table a rough approximation to the variability has been made by taking the mean for the first

* In his paper on *Spinax*, Professor Braus gave the proportion of ♂s to ♀s as 11 : 5 at Ask, and 5 : 6 at Drobak ('99, p. 421). In reply to a letter of mine Professor Braus explained how an error had crept into the MSS. on this point and very kindly sent me the actual figures which I have here made use of.

† Four of these ♂s do not figure in the Appendix Tables, as they were crippled and therefore useless from an anatomical point of view.

five characters given (i.e. the characters determined in both embryos and adults) in each instance. This gives the following figures :—

	Average value of σ for five characters
♂ adults	1·131
♂ embryos	1·174
♀ adults	1·132
♀ embryos	1·057

TABLE 2*.

Comparison of the Standard Deviation (σ) and its Probable Error (P. E. σ) *between the sexes, and between embryos and adults of each sex.*

	♂ adults (100)	♂ embryos (145)	♀ adults (163)	♀ embryos (159)
Anterior spine ... ... σ	·670	·568	·721	·585
P. E. σ	± ·032	± ·023	± ·028	± ·022
Posterior spine ... ... σ	·888	·910	·906	·862
P. E. σ	± ·042	± ·036	± ·034	± ·028
Whole vertebrae ... ... σ	1·147	1·319	1·072	1·093
P. E. σ	± ·055	± ·052	± ·040	± ·041
Half vertebrae ... ... σ	1·901	1·917	1·921	1·702
P. E. σ	± ·091	± ·076	± ·072	± ·064
Total segments ... ... σ	1·048	1·154	1·041	1·045
P. E. σ	± ·050	± ·046	± ·039	± ·039
First girdle-piercing nerve σ	·871	—	·771	—
P. E. σ	± ·029	—	± ·021	—
Post-girdle nerves... ... σ	·819	—	·758	—
P. E. σ	± ·028	—	± ·021	—
Collector nerves ... ... σ	·630	—	·606	—
P. E. σ	± ·021	—	± ·017	—

(1) *Comparison between the adults.* The ♀s appear to be somewhat more variable with regard to the position of the spines and also in the number of half vertebrae, though the difference in each case is not significant. For total segments the variability is almost the same in each sex. The ♂s are more variable for whole vertebrae though here again the difference is not really significant. The average variability is practically identical in each case (1·131 for ♂s and 1·132 for ♀s). For characters, however, which involve the position of the pelvic girdle and plexus the ♂s are somewhat more variable than the ♀s. In the case of the 1st g. p. n. the difference is more probably significant; it may be significant for the post-girdle nerves. The smaller difference in the collector nerves also points to greater ♂ variability. A comparison then of the variability in the adults only of each sex seems to point (α) to equality of variability for characters involving the spines and vertebral column, and (β) to greater variability

* Sheppard's correction has not been used here.

TABLE 3.

Significance of Differences between Embryos and Adults, and between Males and Females.

Δ_{st} = difference of standard deviations of classes s and t.

$R = \Delta_{st}/\sqrt{\text{sum of squares of probable errors of standard deviations.}}$

Test of significance as on p. 318.

Character	♂ embryo — ♂ adult			♀ embryo—♀ adult			♂ adult— ♀ adult			♂ embryo— ♀ embryo		
	Δ	*R*	Significance	Δ	*R*	Significance	Δ	*R*	Significance	Δ	*R*	Significance
Anterior spine	−·102	−2·6	Probable	−·136	−3·8	Highly probable	−·051	−1·2	None	−·017	−·5	None
Posterior spine	·022	·4	None	−·044	−1·0	None	−·018	−·3	None	+·048	+1·1	None
Whole vertebrae	·172	2·3	Probable	·021	·4	None	+·075	+1·1	None	+·226	+3·4	Highly probable
Half vertebrae	·016	·1	None	−·219	−2·3	Probable	−·020	−·2	None	+·215	+2·2	Probable
Total segments	·106	1·6	None	·004	·1	None	+·007	+·1	None	+·109	+1·8	Doubtful
First girdle-piercing nerve	—	—	—	—	—	—	+·100	+2·8	Probable	—	—	—
Post-girdle nerves ...	—	—	—	—	—	—	+·061	+1·7	Doubtful	—	—	—
Collector nerves	—	—	—	—	—	—	+·024	+·9	None	—	—	—

There are thus six characters in which the embryos are more variable than the adults and four in which they are less variable. There are only four in which this difference in variability is probably significant. This significant difference, since the above adults are not the parents of the embryos, may be due to the actual parents being a small selection from the general population. We can only test this in the case of mothers; we then find no significant difference between weighted mothers and actual offspring in the half vertebrae, but a still more significant difference in the anterior spine. Hence it would seem certain that the adults are *more* variable than the embryos in the position of the anterior spine in the case of both sexes. Whether the ♂ embryos are really more variable than the adults in whole vertebrae can hardly be settled from the data.

There are four out of thirteen characters in which the ♀s are more variable than the ♂s, but in no one of these is the difference really significant. There are nine cases in which the ♂s are more variable than the ♀s; in one of these, whole vertebrae, the difference is almost certainly significant for embryos, it is probably significant in half vertebrae for embryos and in first girdle-piercing nerve for adults. In the other cases it is twice hardly significant and four times certainly not significant.

on the part of the ♂s for characters connected with the position of the pelvic fins. See Table 3, p. 323.

(2) *Comparison for embryos.* It has been seen above that the average variability for the five characters taken is much greater for ♂ than for ♀ embryos (1·174 to 1·057). The ♀ embryos show a greater variability for the anterior spine but for all the four remaining characters the ♂s are more variable, and in the cases of whole and half vertebrae (and possibly total segments) the differences are so great in comparison with the probable errors of the differences as to leave little doubt but that they are significant. See Tables 3 and 3 *a*.

(3) *Comparison of embryos with adults.* It has been seen that the average variability is greater for the embryos than for the adults in the case of the ♂s (1·174 to 1·131) and less in the case of the ♀s (1·057 to 1·132). The variability for the different characters is somewhat irregular, but there is a striking agreement between the way in which on the one hand the ♂ embryos differ from the ♂ adults, and the way in which on the other hand the ♀ embryos differ from the ♀ adults. This may be expressed by saying that (*a*) where the ♂ embryos are less variable than the ♂ adults, the ♀ embryos will be less variable than the ♀ adults and to a greater degree; (*β*) where the ♂ embryos and adults are equally variable the ♀ embryos will be less variable than the ♀ adults; and (*γ*) where the ♂ embryos are more variable than the ♂ adults, the ♀ embryos and adults will be equally variable. This point is exemplified in the subjoined table, which gives roughly the comparative variability of the embryos and adults in each sex.

TABLE 3 *a*.

\+ signifies that the embryos are more variable than the adults.
− " " less " "
[ ] signifies that the difference in variability is small and lies within the mean of the probable errors for embryos and adults, i.e. that there is in such cases practically equality of variability.

Comparative variability of	♂ embryo to ♂ adult	♀ embryo to ♀ adult	Amount by which ♂ embryos are more variable than ♀ embryos
Anterior spine ...	− by 3 P. E.*	− by $5\frac{1}{2}$ P. E.	$-2\frac{1}{2}$ P. E.
Posterior spine ...	[+] " $\frac{1}{2}$ P. E.	− " $1\frac{1}{2}$ P. E.	2 P. E.
Whole vertebrae ...	+ " 3 P. E.	[+] " $\frac{1}{2}$ P. E.	$2\frac{1}{2}$ P. E.
Half vertebrae ...	[+] " $\frac{1}{2}$ P. E.	− " 3 P. E.	3 P. E.
Total segments ...	+ " 2 P. E.	[+] " $\frac{1}{10}$ P. E.	2 P. E.

Hence the ♂ embryos as compared with the ♂ adults show a relatively higher variability throughout (except for the anterior spine) than do the ♀ embryos as compared with the ♀ adults. Again, while the ♂ embryos are absolutely more variable than the ♂ adults, the ♀ embryos are absolutely less variable than the

* P. E. here signifies the mean of the two probable errors, i.e. that for embryos and that for adults.

♀ adults. If we suppose the groups of embryos and adults with which we are dealing to be fair samples of the whole population, and there seems no valid reason for any assumption to the contrary*, then it is to some such process as the following that we must look for an explanation. We must suppose that higher variability is an attribute of the ♂, but that, owing to some subsequent process of selection in this sex the variability of adult ♂s is brought down to equality with that found among adult ♀s. This, however, will not serve to explain the lessened variability of ♀ embryos as compared with ♀ adults. It is possible that these differences are due to unconscious selection of the embryonic material which is the product of not more than 40 mothers. On *a priori* grounds it is therefore most unlikely that this relatively small number of mothers is truly representative of mothers as a whole. To test this point the variability of ♀ offspring of known mothers was calculated and compared with the variability of the weighted mothers, and the results given in the following table:—

TABLE 4.

	σ for ♀ embryos ($N=115$)	σ for weighted mothers (Actual No. $=25$)
Anterior spine ...	·6242	·6665
Posterior spine ...	·7447	·6715
Whole vertebrae ...	·9921	·8192
Half vertebrae ...	1·5920	1·7358
Total segments ...	·9903	1·0495
Average ...	·9987	·9885

Hence when there is identical selection of material the average variability of ♀ embryos and ♀ adults is also identical. It is therefore exceedingly likely that the comparatively low variability of ♀ embryos as a whole as compared with ♀ adults as a whole is due to the fact that the ♀ parents were here unconsciously selected. There is every reason to suppose that such a diminution of embryonic variability as the result of unconscious selection obtains equally among the ♂ embryos. The average variability of the ♂ embryos should therefore be appreciably greater than the figure (1·174) already given, thus accentuating the phenomenon of greater variability among ♂ embryos as compared with ♂ adults.

In summing up briefly what is to be learnt from a comparison of the variability of ♂s and ♀s, embryos and adults, it may be tentatively stated that the average variability of ♀ embryos and of adults of both sexes is practically identical, whilst the variability of ♂ embryos is considerably greater. From which follows the

[* The variability of the weighted mothers is in one case equal to that of the adult ♀s. In the four other cases it is less, and in three of them—posterior spine, whole vertebrae and half vertebrae—very significantly less than that of adult ♀s. K. P.]

conclusion that some process of selection takes place among the ♂s during post-natal existence, leading to a reduction of their variability to the amount exhibited by the unselected ♀s. And this conclusion is in accordance with the fact that, whilst the two sexes are produced in equal numbers, the adult ♂s are less numerous than the adult ♀s. (Cf. p. 321.)

Variability in different Regions.

That considerable differences in variability, as measured by the standard deviation, obtain for various regions of the vertebral series has already become apparent. As, however, the number of meristic units which goes to make up these different regions differs widely, their relative variability must be compared by means of the coefficient of variability $\left(=\frac{100\,\sigma}{M}\right)$. These values are given in Table 5 *a*, from which it appears that the variability shows wide differences

TABLE 5 *a*.

Coefficients of Variation.

	♂ adults	♂ embryos	♀ adults	♀ embryos
Anterior spine ...	4·19	3·47	4·46	3·65
Posterior spine ...	2·19	2·24	2·20	2 12
Whole vertebrae ...	2·57	2·96	2·38	2·44
Half vertebrae ...	4·71	4·79	4·77	4·26
Total segments ...	1·61	1·78	1·59	1·61

according to the character chosen—differences on the whole equally well marked in the four groups of embryos and adults. The existence of such differences is antagonistic to the conception of the landmarks along the vertebral series being associated with particular vertebrae, and depending for their various positions on any uniform process of augmentation or reduction taking place in that series. To this point however reference will be made later on (pp. 335–6).

Correlated Variability in the Meristic Series.

With the idea of attempting to throw some light on the processes at work in the vertebral column, the various characters have been correlated with one another. This has been done separately for the two sexes, as well as for embryos and adults apart. The results given in Table 5 *b* show that there is on the whole considerable agreement in the four groups of material. Where the correlation value between two characters is high for one group it usually exhibits a similar high value for

the other three groups, and *vice versâ*. Certain marked discrepancies however are to be found, notably (α) between embryos and adults for the correlation of the anterior with the posterior spine; (β) between the ♂ embryos and the other three groups for the correlation between total segments and half vertebrae; and (γ) between ♀ adults and the rest for the correlation of anterior spine with total segments. Certain of these discrepancies will be referred to later in connection with growth changes (p. 333). Meanwhile some more or less definite conclusions may be drawn from the table of correlation values.

TABLE 5 *b**.

Correlation Values.

	♂ adults	♂ embryos	♀ adults	♀ embryos
Whole vertebrae and posterior spine	·7317 ± ·0304	·7742 ± ·0218	·6540 ± ·0297	·7218 ± ·0248
Total segments and posterior spine	·6753 ± ·0356	·6832 ± ·0290	·6754 ± ·0282	·5882 ± ·0316
Total segments and whole vertebrae	·5534 ± ·0454	·6707 ± ·0299	·5660 ± ·0348	·6492 ± ·0300
Whole vertebrae and half vertebrae	− ·5226 ± ·0476	− ·5580 ± ·0374	− ·4859 ± ·0392	− ·5028 ± ·0388
Anterior spine and posterior spine	·5216 ± ·0485	·2102 ± ·0524	·4828 ± ·0420	·2719 ± ·0485
Total segments and half vertebrae	·3745 ± ·0563	·1945 ± ·0523	·4397 ± ·0414	·3266 ± ·0464
Total segments and anterior spine	·1544 ± ·0645	·1684 ± ·0528	·3279 ± ·0485	·2246 ± ·0493
Anterior spine and half vertebrae	− ·1998 ± ·0635	·0009 ± ·0543	·0031 ± ·0542	− ·068 ± ·0517
First girdle-piercing nerve and post-girdle nerves	− ·6227 ± ·0283	—	− ·5758 ± ·0254	—
First girdle-piercing nerve and posterior spine	·5768 ± ·0309	—	·5398 ± ·0270	—
First girdle-piercing nerve and whole vertebrae	·6181 ± ·0286	—	·4548 ± ·0302	—
First girdle-piercing nerve and collector nerves	·5590 ± ·0318	—	·3821 ± ·0325	—
First girdle-piercing nerve and total segments .	·5051 ± ·0345	—	·4253 ± ·0312	—
Total segments and collector nerves	·2056 ± ·0443	—	·102 ± ·0376	—
No. of fin rays and pelvic nerves	—	—	·058 ± ·0680	—
No. and proportional length of whole vertebrae	·5838 ± ·0582	—	—	—

(1) The correlation of each of the various characters chosen with the total number of segments differs very considerably. Thus, for the posterior spine and for the whole vertebrae it is on the average ·66 and ·61 respectively—for the anterior spine and for the half vertebrae only ·23 and ·33. To take the case of

* Sheppard's correction has not been used here.

TABLE 5 *c*.

Significance of Differences in Correlation Values.

Δ_{st} = Difference between correlations for classes s and t.

$R = \Delta_{st} / \sqrt{\text{sum of squares of probable errors of correlations.}}$

Test of significance as on p. 318.

Pair of characters correlated	♂ adult—♀ adult			♂ embryo—♀ embryo			♂ adult—♂ embryo			♀ adult—♀ embryo		
	Δ	*R*	Significance	Δ	*R*	Significance	Δ	*R*	Significance	Δ	*R*	Significance
Whole vertebrae and posterior spine ...	+·078	+1·8	Doubtful	+·052	+1·6	None	+·043	+1·1	None	+·068	+1·7	Doubtful
Total segments and posterior spine ...	−·000	− ·0	None	+·095	+2·2	Probable	+·008	+ ·2	None	−·087	−2·1	Probable
Total segments and whole vertebrae ...	−·013	− ·2	None	+·022	+ ·5	None	+·117	+2·2	Probable	+·083	+1·8	Doubtful
Whole vertebrae and half vertebrae ...	−·037	− ·6	None	−·055	−1·0	None	−·035	− ·6	None	−·017	− ·3	None
Anterior spine and posterior spine ...	+·039	+ ·6	None	−·062	− ·9	None	−·311	−4·4	Highly probable	−·211	−3·3	Highly probable
Total segments and half vertebrae ...	−·065	− ·9	None	−·132	+1·9	Doubtful	−·180	−2·3	Probable	−·113	−1·8	Doubtful
Total segments and anterior spine ...	−·174	−2·2	Probable	−·056	− ·8	None	+·04	+ ·2	None	−·103	−1·5	None
Anterior spine and half vertebrae ...	−·203	−2·4	Probable	+·069	+ ·9	None	+·201	+2·4	Probable	−·071	− ·9	None
1st girdle-piercing n. and post-girdle nerves	−·045	−1·2	None	—	—	—	—	—	—	—	—	—
1st girdle-piercing n. and posterior spine	+·037	+ ·9	None	—	—	—	—	—	—	—	—	—
1st girdle-piercing n. and whole vertebrae	+·163	+3·9	Highly probable	—	—	—	—	—	—	—	—	—
1st girdle-piercing n. and collector nerves	+·177	+3·8	Highly probable	—	—	—	—	—	—	—	—	—
1st girdle-piercing n. and total segments	+·080	+1·7	None	—	—	—	—	—	—	—	—	—
Total segments and collector nerves ...	+·104	+1·8	Doubtful	—	—	—	—	—	—	—	—	—

This table shows that sexual difference is only significant in five out of the twenty-two cases dealt with. Of these five, it is 'probable' in three and 'highly probable' in two. There is sensibly no difference in correlation between the embryos of the two sexes, and the most important cases of the adults are those for the first girdle-piercing nerve with the whole vertebrae and with the collector nerves, cases for which we have not data for the embryos. There appear to be markedly significant differences for both sexes between adults and embryos in the correlation between posterior and anterior spines, the correlation intensifying with age. There are three other cases of probable significance in the male, and one only in the female, although in the latter sex the correlations increase with age in six out of the eight cases.

the two spines—the fact that the average correlation with the total number of segments is nearly three times as great for the posterior as for the anterior would seem to point conclusively to dissimilarity in the factors determining their position*. Certainly they cannot owe their various positions to some uniform process of shortening or lengthening of the vertebral column alone†. Probably one must regard the spines as structures showing oscillations from a mean position backwards and forwards along the vertebral column. These oscillations are relatively greater in the anterior spine (where the C.V. is about 4), and are more independent of the total number of segments because there are no specialized structures in close proximity tending to fix its position. Consequently the correlation with the total number of segments is small. The posterior spine on the other hand occurs in the region of the pelvic fins and of the junction of the whole with the half vertebrae. With the number of the whole vertebrae, and with the position of the pelvics as estimated by the 1st g. p. nerve, it is highly correlated (the average values of these correlations being about ·72 and ·55 respectively). The coefficient of variability for the posterior spine is smaller than that for the anterior, which is probably to be explained by the fact that the high correlations alluded to tend to limit the oscillations of the former structure. These high correlation values force us to conclude that the three structures concerned vary very much in sympathy with one another. The fact that the position of the posterior spine is largely dependent on the number of whole vertebrae and on the position of the pelvic fins must lead us to suppose that its independent oscillations are to a great extent checked, a view that is borne out by the relatively low variability (average C.V. = 2·19) of this spine as compared with that of the anterior. Moreover, the intimate relation of the posterior spine with the whole vertebrae serves to explain its high correlation with the total number of segments. For the number of the whole vertebrae is largely dependent on the total number of segments, as the average correlation value ·61 shows. To sum up, the position of the spines was probably in the first place largely independent of the total number of segments, and, where no further complications are introduced, remains so, as in the case of the anterior spine. When, however, the spine enters into close relation with other structures whose position is largely dependent on the total number of segments, its variability becomes checked and the value of its correlation with the last-named feature increased.

(2) As the vertebral column is composed partly of whole and partly of half vertebrae, one would expect any change in the total number of meristic units to affect both of these regions. This is the case, though not to an equal extent in each. A high correlation exists between total segments and whole vertebrae—a considerably lower one between total segments and half vertebrae. Whilst the

* On the hypothesis of random interpolation the ratio of these correlations should be the ratio of the standard deviation of the posterior spine to anterior spine, or on the average ·892 to ·636, i.e. rather as 3 to 2, than the observed 3 to 1.

† Nor to random interpolation.

number of whole vertebrae, the meristic units characteristic of the trunk region, is largely governed by the total number of segments (average correlation = ·61), the number of the more variable half vertebrae constituting the caudal region is far less dependent upon this factor (average correlation = ·33). From this difference* it must be inferred that the influence of any factor leading to an increase or decrease in the total meristic series will not be equally exhibited in the change in number of the whole and of the half vertebrae. Thus, reduction of the total segments will be accompanied by a marked reduction in the number of whole vertebrae, and by a much smaller one in the number of half vertebrae. Hence, the result of increasing reduction of the total number of segments, and consequently of the whole vertebrae, must be to increase the ratio $\frac{\text{number of half vertebrae}}{\text{number of whole vertebrae}}$. That such is actually the case is strikingly brought out on correlating the numbers of the half and whole vertebrae. The correlation value (Table 5 *b*) is fairly high (average = ·52) and is *negative*. In other words, the greater the number of whole vertebrae the *relatively* smaller is the number of half vertebrae associated with them. Elsewhere (p. 334) reasons are given for supposing that a gradual reduction in the number of segments is in process phylogenetically. The above facts would lead one to suppose that this is being brought about by reduction of the whole vertebrae, owing to their transformation into half vertebrae, and the variations already observed (p. 315) at the point of junction lend colour to this view. At the same time the number of half vertebrae is being reduced caudally but recruited, though to a somewhat less extent, rostrally from the whole vertebrae. Were the reduction in the number of whole vertebrae to keep pace with the reduction in the total number of segments, the correlation between these two characters would be unity. The number of half vertebrae would however be recruited by an increased amount, and its correlation with the total number of segments correspondingly reduced. Hence, on this view we should be led to expect that the higher is the correlation between the whole vertebrae and total segments, the lower will it be between the total segments and half vertebrae. If we consider the sexes separately this expectation is borne out by the actual numbers. Table 5 *b* shows that the correlation between whole vertebrae and total segments is slightly higher in the embryos of each sex than in the corresponding adults, whilst at the same time the corre-

* The extent to which the whole and half vertebrae respectively would be affected by a change in the total number of segments is more correctly given by the coefficient of regression of either of these two characters on the total number of segments. Thus the value of the regression of whole vertebrae on the total segments is given by the expression $\frac{\sigma \text{ for whole vertebrae}}{\sigma \text{ for total segments}} \times$ correlation between whole vertebrae and total segments. In the case of adult ♂ s this is $\frac{1{\cdot}147}{1{\cdot}048} \times {\cdot}553 = {\cdot}602$. Similarly the value of the regression of half vertebrae on total segments is $\frac{1{\cdot}002}{1{\cdot}048} \times {\cdot}375 = {\cdot}353$. These two values show clearly that any change in the number of the total segments affects the whole vertebrae much more than the half vertebrae. (The value of σ for half vertebrae is 1·901 (Table 2) but this must be first translated into terms of total segments (cf. App. Table 27) which gives the above value 1·002.)

lation between total segments and half vertebrae is considerably lower in the embryos (cf. p. 327)*.

(3) The position of the pelvic fin (as determined by the 1st g. p. nerve) is in either sex fairly highly correlated with the posterior spine, the number of whole vertebrae, and the total number of segments. The value of the last correlation (average = ·46) is however sufficiently low to show that the position of this fin cannot be entirely due to any uniform process of reduction or increase in the meristic series. It has already been seen that, of the total fin innervation, part—the collector—is rostral to the pelvic girdle, and part—the post-girdle nerves—is caudal to that structure. On the supposition that the girdle is, so to speak, in a state of oscillation (exhibiting either backward or forward homoeosis, but especially the former), we should be led to look for evidence of such oscillation in the values of the correlations between these two sets of nerves and the position of the 1st g. p. nerve. We should expect an oscillation resulting in a more rostral position of the girdle to be accompanied by a diminution in the number of the collector branches and an increase in the number of the post-girdle nerves. This expectation is fully borne out by the correlations. That between the 1st g. p. nerve and the post-girdle nerves is high (− ·62 for ♂s and − ·57 for ♀s) and is *negative*. The smaller the number denoting the 1st g. p. nerve (i.e. the more rostral the

[* It may be of slight interest to consider the topic of this paragraph from a rather more mathematical aspect.

The average correlation between whole vertebrae and total segments is ·610 and between half vertebrae (in whole vertebrae as units) is ·334. The mean standard deviation of total segments is 1·072, of whole vertebrae 1·158, and of half vertebrae (in whole vertebrae as units) is 1·096. Hence forming the regression coefficients:

$$\frac{\text{S. D. for whole vertebrae}}{\text{S. D. for total segments}} \times \text{correlation } \cdot 610, \text{ and } \frac{\text{S. D. for half vertebrae}}{\text{S. D. for total segments}} \times \text{correlation } \cdot 334,$$

we see that on the average an increase of one in the total segments will be accompanied by an increase of ·66 in the whole vertebrae and an increase of ·34 in the half vertebrae (measured in whole vertebrae as units). In other words one new whole vertebra is as likely to appear as one new half vertebra (measured as a half vertebra). But the total number of whole vertebrae is about double (44·6 to 20·4) the number of half vertebrae (in whole vertebra units); in other words the effect of increasing or decreasing the total segments is relatively to the whole and half vertebrae sensibly the same as adding or subtracting a vertebra anywhere at random, because the total number of whole vertebrae is roughly twice as great as the half vertebral series measured in the same unit, the whole vertebra.

Let w = whole vertebrae, h = half vertebrae (measured in whole as units), t = total segments, r = correlation coefficient. Then if the total number of segments were constant, since the standard deviations of whole and half vertebrae are approximately equal, we should expect that the expression for partial correlation, i.e.

$$\frac{r_{wh} - r_{wt} r_{ht}}{\sqrt{(1 - r^2_{wt})(1 - r^2_{ht})}},$$

should be closely equal to minus unity, for when the whole vertebrae lose one the half vertebrae must gain one. Equating the above expression to -1, and noting that $r_{wt} = \cdot 610$ and $r_{ht} = \cdot 334$ nearly, we find $r_{wh} = -\cdot 54$, the average observed value of r_{wh} is $-\cdot 52$—a very close result. In other words the correlation of whole and half vertebrae is what we might reasonably expect its value to be, the correlations for the two parts with the total being as above. In general terms the result is the same as if 50 per cent. of gain or loss of either whole or half vertebral series came from increase of total segments and 50 per cent. from a transfer from one series to the other. K. P.]

position of the girdle), the larger the number of post-girdle nerves associated with that position.

A considerable discrepancy will be noticed in the two sexes for the value of the correlation between the 1st g. p. n. and the collector nerves. The explanation is probably somewhat as follows. It has been shown by Braus (**'01**, p. 620) that the number of collector nerves in *Spinax* is greater during earlier than in subsequent stages of development. Even in early stages this rostral extension of the collector area has its limits. It is natural to suppose that with forward shifting of the girdle the more rostral of these branches will persist as the rostral part of the nervus collector. As however no nerve whose serial number is less than 21 seems to take part in the formation of the collector during early stages, any marked backward homoeosis in the pelvic area will be associated with a correspondingly marked reduction of the collector nerves. As the girdle shifts forwards the reduction in the number of collector branches will become disproportionately greater. Consequently, where the rostral pelvic shifting is less, we should expect to find a smaller correlation between the 1st g. p. nerve and the collector nerves. And this is actually what takes place. The ♀s, with their more caudally situated pelvics and greater average number of collector nerves, show a correlation value of only ·38, whilst that for the ♂s, where the girdle is more rostrally placed and the number of collector nerves somewhat fewer, amounts to ·55.

(4) It will be noticed in Table 5 *b* that certain correlation values are very low. This is the case for those between the anterior spine and half vertebrae, between the total number of segments and the collector nerves, and between the fin rays and the pelvic nerves. On the hypothesis that some uniform process of excalation is going on the mean correlations for the above characters ought not to differ greatly from the mean of the rest, which is somewhere between ·5 and ·6. Especially should there be a high correlation between the number of fin rays and the fin nerves. On the side-fold theory of limb origin the morphological connection between these structures is so intimate that a high correlation between the two would naturally be looked for, and the fact that there is practically no correlation whatever tells strongly against this theory. Moreover the little that is known on this head for *Acanthias*, the only other Elasmobranch similarly investigated (**'01**, p. 13), is in accordance with the foregoing facts.

(5) A well-marked correlation exists between the number of the whole vertebrae and their total length relative to the whole vertebral column. As will be seen (in Appendix, Table 36) the length of the series of whole vertebrae varies between ·605 and ·695 of the whole vertebral column. The range is consequently ·09, i.e. about 1/7·5 of the length of the series, whilst the range for the number of whole vertebrae is 6 in a series of 48, i.e. $\frac{1}{8}$. The range of variation in respect of length therefore bears approximately the same relation to the relative length as the range for number does to the total number of whole vertebrae. The value of the correlation is fairly high but should be very much closer to unity if the number is entirely dependent upon the length.

Growth Changes.

Note was made above (p. 327) of certain marked discrepancies in the correlation values.

(1) The correlation between anterior and posterior spines is much lower in embryos than in adults. Both anterior and posterior spines are rather more rostrally situated in the embryos than in the adults. The differences, however, in the ♂s are small and will not serve to explain the very low correlation values of the embryos. Again the variability of the posterior spine is very close in all the groups. When, however, the variability of the anterior spine is examined it is at once evident that its value is much less for the embryos than for the adults. It would appear not unlikely that changes in the position of this spine occur during post-natal growth. As these changes are in the direction of higher correlation with a more variable structure, the posterior spine (as estimated by σ), they must lead to greater variability in the anterior spine. Such is actually the case. Consequently we must suppose that the anterior spine is subject to a certain amount of change in position during growth. And in this connection it is interesting to notice that greater variability is associated with higher correlation also in the case of the characters involving the pelvic nerves of the ♂s as compared with the ♀s (cf. Tables 2 and 30—35).

(2) The correlation between the total segments and the half vertebrae for ♂ embryos is very low. The correlation between the total segments and whole vertebrae in this same group is however markedly higher than in the rest. Here again it is probable that growth changes come in. If during post-embryonic life we suppose that there is a certain amount of transformation of whole vertebrae into half vertebrae, such a process would lead to a reduction in the value of the correlation between whole vertebrae and total segments and an increase in the value of the correlation between total segments and half vertebrae. This is actually what has come about in the adult ♂s. Moreover the correlation values bear out the supposition that the same process occurs, though to a less extent, among the ♀s. The correlation between the half vertebrae and total segments is less in the embryos than in the adults, whilst that between the whole vertebrae and the total segments is greater. It is therefore exceedingly probable that a certain amount of conversion of whole into half vertebrae occurs in either sex during post-natal existence.

Variability in the Girdle Canals.

That there may be either one or two canals in the pelvic girdle for the transmission of nerves has already been noted. The figures below seem to show without doubt that the number of the canals is connected with the position of the girdle.

		No. of cases *	Mean position of girdle
♂	1 canal	110	28·69
	2 canals	86	28·17
	Difference		·52
♀	1 canal	184	29·17
	2 canals	112	28·86
	Difference		·31

The possession of two canals is associated with a markedly more rostral position of the girdle, the difference being half a metamere for the ♂s and a third of one in the case of the ♀s. Now it has already appeared that the pelvic girdle is more rostrally situated in the ♂ than in the ♀. Consequently we should be led to expect a larger proportion of fish with two canals among the ♂s, and this condition is what actually obtains. The $\frac{\text{no. of ♂s with 2 canals}}{\text{no. of ♂s with 1 canal}}$ is $\frac{86}{114} = \frac{78{\cdot}2}{100}$, whilst the $\frac{\text{no. of ♀s with 2 canals}}{\text{no. of ♀s with 1 canal}}$ is $\frac{112}{184} = \frac{60{\cdot}9}{100}$, cases of two canals being distinctly more numerous among the ♂s. Reasons have already been given (p. 331–2) for supposing that the different positions of the pelvic girdle are, at any rate in the main, due to a process of homoeosis, and that such homoeosis is for the most part backward homoeosis (cf. Bateson, '**94**, pp. 111 *et seq.*). Now the preponderance of two canals among the more rostrally situated girdles is explicable on the supposition that this homoeosis is in some measure ontogenetic. The "Anlage" of the girdle is relatively larger than its ultimate condition. Its width is somewhat greater than the interval between two successive limb nerves, some one of which it must necessarily enclose. Supposing then that rostral migration of the girdle occurs, that structure will at first abut against and ultimately enclose the nerve immediately anterior to it, whilst at the same time retaining as it were its grasp of the nerve which orginally perforated it. That some such process does actually occur is rendered probable by the observations of Braus ('**01**, p. 590 and Taf. XXIII, Fig. 8) on the arrangement of the cartilage cells. Hence the variations in the number of the girdle canals support the view that the different positions of the girdle are largely due to homoeosis, and that such homoeosis is to some extent ontogenetic, though this is probably confined to the earlier stages of development.

The Hypothesis of Excalation.

In most linear meristic series certain members become specialized in a definite manner and acquire thereby an individuality that serves to distinguish them from the rest of the series. In *Spinax* such specialization occurs in the case of those vertebrae which enter into a definite relation with the fins, paired and unpaired. On the hypothesis under consideration it is supposed that such specialized

* As the number of canals may differ on the two sides of the same animal each side has been considered separately as a "case."

structural peculiarities become as it were the fixed attribute of certain particular segments. Thus in *Spinax* the vertebra that carries the anterior spine is to be regarded as strictly homologous in different specimens. The serial position of such a segment may vary in different individuals—in one it may be the *n*th, in another the $(n+1)$th, &c. On the hypothesis of excalation it is supposed that creatures such as *Spinax* have been derived from forms with a larger number of segments and that the various positions of the segments with a marked individuality are due to a uniform process of shortening up owing to the vertebral column splitting into a lessened number of parts. The amount of change in position in the specialized segment depends partly on the amount of reduction or excalation in the meristic series of which it is a member, and partly on its original position in that series. As a simple illustration we may take the case of a series consisting of 40 segments, the 10th and 20th of which are specialized, let us say by bearing spines. If we suppose the series to be reduced to 36 segments by a uniform process it is obvious that the spine-bearing segments will now be the 9th and 18th. Reduction of the whole series to the extent of four segments implies a change in position of the anterior spine to the extent of one segment, and in the posterior spine of two segments. In other words, the nearer to the middle of the series the specialized segment lies the greater is the change in its position involved by reduction in the number of the whole series. On the excalation hypothesis we are led to expect (1) a positive correlation between each specialized segment and the total number of the meristic series, and (2) approximate equality in the value of these correlations. Reference to Table 5 *b*, p. 327, shows that the former of these expectations is fulfilled. The value of the correlation between posterior spine and total segments is however very much greater than that between total segments and anterior spine, whilst the value of the correlation between another specialized segment, that of the 1st g. p. nerve, and the total number of segments is intermediate. Such facts throw considerable doubt on the possibility of symbolizing the facts by the excalation theory and the following considerations serve to emphasize that doubt.

TABLE 6 *a*.

A. *Posterior Spine.*

Number of segments	...	62	63	64	65	66	67	68
Number of specimens	...	2	26	91	158	81	33	5
Calculated values for position of posterior spine		38·9	39·6	40·2	40·8	41·4	42·1	42·8

The mean calculated position of posterior spine is **40·8** and is associated with 65 segments, which is the mean of the total number of segments. The standard deviation for the posterior spine (calculated position) works out to ± ·678.

Observed values for position of posterior spine:

		38	39	40	41	42	43	44
Number of specimens	...	1	21	124	156	81	11	2

Mean position = **40·8**, $\sigma = \pm$ ·942.

TABLE 6 *b*.

B. *Anterior Spine.*

Number of segments ...	63	64	65	66	67	68
Number of specimens ...	8	58	140	113	52	9
Calculated values for position of anterior spine	15·5	15·75	16	16·25	16·5	16·75

Then $\sigma = \pm \cdot 284$.

Observed values for position of anterior spine:

	14	15	16	17	18
Number of specimens ...	2	56	256	60	6

Whence $\sigma = \pm \cdot 624$.

TABLE 6 *c*.

C. *First girdle-piercing Nerve.*

♀s (296)

Number of segments ...	63	64	65	66	67	68
Number of specimens ...	4	44	120	80	42	8
Calculated values for position of first girdle-piercing nerve	27·89	28·34	28·78	29·22	29·66	30·11

Mean number of segments = **65·5**; mean of first girdle-piercing nerve = **29·0**.

Whence $\sigma = \pm \cdot 461$.

Whilst observed value of $\sigma = \pm \cdot 760$.

♂s (168)

Number of segments ...	63	64	65	66	67	68
Number of specimens ...	10	38	74	30	14	2
Calculated values for position of first girdle-piercing nerve	27·62	28·06	28·50	28·94	29·38	29·82

Mean number of segments = **65·0**; mean of first girdle-piercing nerve = **28·5**.

Whence $\sigma = \pm \cdot 458$.

Whilst observed value of $\sigma = \pm \cdot 879$.

If a number of specimens of *Spinax* are taken, in each of which the number of total segments is known, we may, knowing also the mean number of total segments and the mean position of the posterior spine, calculate the theoretical value of the variability (σ) of the posterior spine on this hypothesis. This has been done for the characters anterior and posterior spine and 1st g. p. nerve in ♂s as well as ♀s, and the results are given in Table 6*. For the posterior spine this theoretical value works out to $\pm \cdot 678$, which is considerably less than the observed value $\pm \cdot 942$. The variability of the posterior spine is actually much greater than it should be on the excalation hypothesis. This is also true for the anterior spine where the observed value, $\pm \cdot 624$, is very much greater than the calculated value $\pm \cdot 284$. The same is also true for the 1st g. p. nerve where the calculated values

* These correlations were worked out in 1902 on the material collected in 1901 and 1902. It has been thought unnecessary to recalculate these figures by incorporating the material collected in 1903. The differences that would be introduced into the above values would probably be very slight.

$\pm$ ·458 and $\pm$ ·461 for ♂s and ♀s respectively fall much below the observed values $\pm$ ·879 and $\pm$ ·760. It is therefore evident that the hypothesis of excalation will not serve to account for the magnitude of the observed variability.

On Intercalation.

By some morphologists (cf. Baur, '**97**, p. 52) it has been held that intercalation of segments as well as excalation may go on in a linear meristic series. To take the case of a portion of a vertebral column between two specialized and homologous segments, there are often to be found individuals exhibiting either a less or a greater number of segments than the normal. The former condition we are called upon to regard as the result of excalation, the latter as being due to intercalation; and to account for variations any such demarcated region of the segmented series must be held capable of undergoing intercalation in one specimen and excalation in another. In order to test this possibility we may consider the case of that portion of the vertebral column which lies between two highly specialized segments,

TABLE 7.

Correlation of Segments lying between First Girdle-piercing Nerve and Posterior Spine with Segments giving off Post-girdle Nerves to Fin.

a. ♂s.

	10	*11*	*12*	*13*	*14*	
8	—	2	3	—	—	5
9	2	8	25	8	1	44
10	1	1	16	10	—	28
11	—	—	1	5	—	6
	3	11	45	23	1	83

$r = ·343 \pm ·063$

b. ♀s.

	10	*11*	*12*	*13*	*14*	
6	—	1	2	—	—	3
7	—	8	14	1	—	23
8	1	11	31	17	3	63
9	—	2	3	8	4	17
10	—	—	1	—	1	2
	1	22	51	26	8	108

$r = ·370 \pm ·056$

viz. the segment whose nerve is the most rostral of those piercing the girdle, and the segment which carries the posterior spine. This division of the vertebral column consists of some 10—14 segments, of which the greater number (nearly three-quarters) are characterized by the fact that their spinal nerves send off branches to the pelvic fin. Now if the variations in the number of these 10—14 segments are due to intercalation and excalation we should expect the variations of those of them which supply the fin muscles to be in sympathy. In other words we should look for a high correlation between the segments bounded by the 1st g. p. nerve and the posterior spine and those which give off the post-girdle fin nerves. These correlations (Tables 7 *a* and *b*) have been worked out for the ♂ and ♀ adults and the values obtained are ·343 and ·370 respectively. On the hypothesis under consideration we should expect these correlations to approximate to unity and the lowness of the values actually obtained tells strongly against the

hypothesis of intercalation. Lastly there is to be considered the possibility that the variations in the number of the meristic series and of its various portions are due to random addition to or subtraction of segments from a normal number. This is a point which, though of the greatest interest to students of variation, is yet extremely difficult for the biologist to test. In the following paragraphs Prof. Pearson has very kindly undertaken this task. It is sufficient here to emphasize his conclusion that the observed correlations are not in agreement with the correlations calculated upon the hypothesis under consideration. In other words random interpolation or excision of segments will not explain the variations observed, and we are consequently forced to adopt the principle of homoeosis as the only conception hitherto offered which affords an explanation of all the facts.

[*On the Random Increase and Decrease of Segments and on the Correlations between the three Vertebral Regions of Spinax niger.*

By Karl Pearson.

I must first state that I do not fully grasp either the hypothesis of excalation or homoeosis, or the manner in which biometric analysis can be used as a criterion between them. But we can, I think, ask how far the *existing* correlations are in keeping with:

(i) the proportional insertion of segments into a series of three mean groups;

(ii) the random insertion of segments into the same groups.

Let x = number of segments up to anterior spine, which we will call the anterior series.

y = number between anterior and posterior spines, which we will call the median series.

z = number beyond posterior spine, which we will call the posterior series.

t = total segments $= x + y + z$.

Let us suppose all these quantities measured in whole vertebrae as units.

Let the mean numbers be $\bar{x}$, $\bar{y}$, $\bar{z}$, $\bar{t}$. Suppose variations ξ, η, ζ, τ to occur in these numbers, so that

$$\tau = \xi + \eta + \zeta.$$

Then if τ were always distributed in any definite proportions whatever between the three groups, say:

$$\xi = p_1\tau, \quad \eta = p_2\tau, \quad \zeta = p_3\tau,$$

where $p_1 + p_2 + p_3 = 1$, we should have

$$\sigma_x = p_1\sigma_\tau, \quad \sigma_y = p_2\sigma_\tau, \quad \sigma_z = p_3\sigma_\tau,$$

or:

$$\sigma_\tau = \sigma_x + \sigma_y + \sigma_z,$$

$$r_{yz} = r_{xz} = r_{xy} = r_{xt} = r_{yt} = r_{zt} = 1.$$

These are manifestly inconsistent with the observed values. For adult ♂s I find:

$$\bar{x} = 15{\cdot}979,\ \sigma_x = {\cdot}670,\ \text{C. of V. for } x = 4{\cdot}194,$$
$$\bar{y} = 24{\cdot}561,\ \sigma_y = {\cdot}785,\ \text{„ „ } y = 3{\cdot}198,$$
$$\bar{z} = 24{\cdot}500,\ \sigma_z = {\cdot}794,\ \text{„ „ } z = 3{\cdot}240,$$
$$\bar{t} = 65{\cdot}040,\ \sigma_t = 1{\cdot}048,\ \text{„ „ } t = 1{\cdot}611,$$

which are far from satisfying the above relations.

Clearly: (i) judged either by the standard deviations or by the coefficients of variation the median and posterior series are equally variable, (ii) the anterior series is absolutely less variable than the median or posterior series, but relatively more variable, and (iii) absolutely the whole series is more variable than any subseries only in the ratio 4 to 3, and relatively it is far less variable.

The existing correlation and variation values are not given by any distribution of insertions and withdrawals in which the ratio of the changes in the subgroups to the total change remains constant.

Next suppose the segments inserted at random, there being no relation whatever between the numbers inserted in any of the subseries. We should then have

$$r_{xy} = r_{xz} = r_{yz} = 0,$$
$$r_{xt} = \sigma_x/\sigma_t,\ r_{yt} = \sigma_y/\sigma_t,\ r_{zt} = \sigma_z/\sigma_t,$$
$$r_{x+y,\,t} = \sigma_{x+y}/\sigma_t, \qquad r_{x,\,x+y} = \sigma_x/\sigma_{x+y}.$$

But $\sigma_{x+y} = {\cdot}888$. Hence we should expect, with the observed values of the variabilities:

$$r_{xt} = {\cdot}6393,\ r_{yt} = {\cdot}7494,\ r_{zt} = {\cdot}7573,\ r_{x+y,\,t} = {\cdot}847,\ \text{and}\ r_{x,\,x+y} = {\cdot}7550.$$

Actually we have:

$$r_{yz} = +{\cdot}0671,\ r_{xt} = {\cdot}1544,\ r_{x+y,\,t} = +{\cdot}6753,$$
$$r_{zx} = -{\cdot}3797,\ r_{yt} = {\cdot}6318,\ r_{x,\,x+y} = +{\cdot}5216,$$
$$r_{xy} = -{\cdot}2633,\ r_{zt} = {\cdot}5648,$$

which differ very sensibly from the above values.

Clearly additional segments are inserted in the three subseries in a correlated manner, and the existing series could not result from a mere random insertion of segments, or a proportioned insertion (nor of course by like withdrawals). Whether the original variation was of one of these kinds and the result was then modified by selection, it does not seem possible to assert on the basis of a statistical examination of *one* race at *one* epoch.

We can of course analyse the existing relationships; we have the following tables:

Regression Coefficients.

Gives in	Change of a unit in			
	x	y	z	t
x	1	−·225	−·321	·099
y	−·309	1	·066	·473
z	−·450	·068	1	·428

The following are the percentage changes in x, y, z:

Gives percentage change in	Change of a unit in			
	x	y	z	t
x	—	−1·41	−2·01	·62
y	−1·26	—	+ ·27	1·93
z	−1·84	+ ·28	—	1·75

These results appear of considerable interest. We see (i) that a gain of one in the total segments makes on the average five times as much absolute effect, and three times as much percentage effect on the median and posterior as on the anterior series: (ii) that a gain in either median or posterior series is accompanied by a very small gain in either posterior or median series respectively, but in both cases by a sensible loss in the anterior series. (iii) Conversely a gain in the anterior series is associated with sensible losses in median and posterior series; and (iv) gains and losses in anterior and posterior series have the highest relation, i.e. the loss of a segment in the anterior series is accompanied by a considerably greater gain in the posterior series than in the median series, and the gain of a segment by a greater loss in the posterior than in the median series. This more intimate relation of the extreme series than of either with the median series seems of considerable suggestiveness.

Lastly I have worked out the partial correlation coefficients of the three subgroups on the hypothesis that the total number of segments remains constant, i.e. expressions like:

$$R_{xy} = (r_{xy} - r_{xt}r_{yt})/\sqrt{(1 - r_{xt}^2)(1 - r_{yt}^2)}.$$

I find

$$R_{xy} = -\cdot4712, \quad R_{xz} = -\cdot5727, \quad R_{yz} = -\cdot4530.$$

Or we find a sensible negative correlation nearly equal to −·5 between all three series. That is to say that if individuals of the same number of total segments be

taken, we do not find all the subgroups either constant or again independent, but since the variabilities of the three are not widely different, the loss of a segment by one is roughly on the average an equally divided gain to the other two. For example if the anterior series lose a segment, this is not added wholly to the median series, but rather more than the half goes on the average to the posterior series. In other words *for fish of the same total number of segments* a forward movement of the anterior spine will on the average be associated with a forward movement of the posterior spine through somewhat more than half the number of segments through which the anterior spine moves.

It would thus appear that the existing system of variations and correlations is not consistent with any definite position of the two spines for fish of a constant number of segments; it is not consistent with any system of independent shifting of the spines about their mean positions in the same fish. It is only consistent with an associated motion of the two spines in the same direction, their average motions having a simple ratio. Thus, while the division between whole and half vertebrae (see ftn. p. 331) appears to vary in a manner not inconsistent with random interpolation into the total series, the relation between the three groups determined by the two spines appears to be of a wholly different character.]

III. Heredity of Meristic Characters.

Shortly after the collection of the present data was begun the idea suggested itself that, owing to the viviparous nature of *Spinax*, such data might be used for the determination of fraternal and parental correlations. For purposes of fraternal correlation, 230 embryos belonging to 27 families (110 ♂s and 120 ♀s, cf. Appendix, Table 13, Nos. 75—304) are available. As the ♀ parent was not determined in Nos. 163—168 the number of embryos of which use can be made for calculating parental correlations is 224, belonging to 25 families. The actual calculation of the correlation coefficients, involving correction for the selection of both parents, was very kindly undertaken by Dr A. Lee in Prof. Karl Pearson's Laboratory, and the results are given in Table 8. The ♂ parents are of course unknown, but their influence has been deduced from the data given for ♂ adults on the assumption that the selection of ♂ parents from the adult ♂ population is equal to that of ♀ parents from adult ♀s.

Fraternal Correlations.

A glance at Table 8 at once brings out two points of interest, viz. (i) the diversity of the correlation values for the five characters treated, and (ii) the lowness of the average value even after parental selection has been corrected for.

(*a*) The diversity of the correlation values is very considerable, being as high as ·447 for the whole vertebrae, and as low as ·254 for the half vertebrae. Moreover

when the sexes are considered separately these differences still obtain (cf. Appendix, Tables 38 to 42). Thus, in the case of total segments, the uncorrected value for ♂s is ·395, for ♀s ·410, and for siblings ·429, values which are all very close together. In one case however, that of the whole vertebrae, the value for the ♂s, i.e. ·541, is considerably higher than that for the ♀s, i.e. ·371. An examination of Table 40

TABLE 8*.

Fraternal Correlations.

	Raw value	Corrected (a)	Corrected (b)
Anterior spine ...	·309 ± ·014	·320	·331
Posterior spine ...	·373 ± ·013	·394	·414
Whole vertebrae ...	·425 ± ·012	·436	·447
Half vertebrae ...	·228 ± ·014	·241	·254
Total segments ...	·429 ± ·012	·425	·422
Mean	·353	·363	·374

(a) Corrected for selection of mothers out of adult ♀ population.

(b) Corrected for selection of fathers also, on the assumption that they although unknown were equally selected with mothers.

shows that this is largely due to the exceptionally close correlation between the six ♂ offspring of ♀ 159 (Table 13, Nos. 261, 263—5, 267, 268), in all of which the number of whole vertebrae is unusually low. In the ♀s of the same family the number of whole vertebrae approximates more closely to the normal. There can be no doubt but that this small group of ♂s is largely responsible for the relatively high value of the fraternal correlation for whole vertebrae.

(*b*) The average value of the five correlated characters works out after correction to ·375. This value is very much lower than the ·5 that the Law of Ancestral Heredity would have led us to expect†, and is markedly lower than the ·475 for eye-colour in man—the lowest value hitherto recorded (cf. Pearson, '**03**, p. 390). In this connection, however, there is a contingency which must not be overlooked, viz. the possibility that a ripe ♀ may be fertilized by more than one ♂. If this be so it would obviously lower the value of the fraternal correlation. We are entirely ignorant of the breeding habits of *Spinax*, but the following circumstantial evidence, slender though it is, seems to tell against the above conjecture. In the virgin ♀ the openings of the oviducts are occluded by the so-called hymen which must be ruptured before fertilization can be achieved.

* Sheppard's correction has been made use of in the three following Tables 8—10.

† [The Law of Ancestral Heredity in no way fixes the value of the fraternal correlation, but only the ratios of *parental* correlation to that of higher ancestors. A fraternal value ·5 to ·6 is found *by observation* on *long* series in man, horse, dog, etc. Galton's data gave ·375 for stature in brother and sister. K.P.]

This is doubtless effected by the claspers of the ♂, powerful intromittent organs deeply grooved for the conveyance of the seminal fluid, and armed with strong spines. Their structure, taken in conjunction with the absence of an ejaculatory apparatus, points to copulation being no momentary action. Now, in several specimens of *Spinax*, I was able to examine the embryos in a very early condition shortly after the appearance of the medullary folds. Such early embryos were all at approximately the same stage, a fact which, considered in relation with what precedes, points to fertilization by a single ♂ only. Further reference to the low values of the fraternal correlations will be made later.

Absence of Differential Sex Variability in Mothers.

Whether ♀s of a given type tend to produce an excess of either sex is a question which the present data allow of an attempt at answering. The ♀ parents were weighted for each character with reference to ♂ and ♀ offspring separately, and the means and standard deviations of these two sets of weighted mothers were calculated. The results are given in Table 9.

TABLE 9.

Showing Variability of Mothers weighted for ♂ and ♀ Offspring respectively.

{Number of ♂ Offspring = 109; number of ♀ Offspring = 115.}

		M.	P. E. M.	σ	P. E. σ
Anterior spine	♂	16·284	±·045	± ·7051	±·032
	♀	16·322	±·042	± ·666	±·030
Posterior spine	♂	41·082	±·046	± ·718	±·033
	♀	41·035	±·043	± ·672	±·031
Whole vertebrae	♂	45·248	±·056	± ·867	±·040
	♀	45·156	±·051	± ·819	±·036
Half vertebrae	♂	39·853	±·105	±1·619	±·074
	♀	39·852	±·109	±1·739	±·077
Total segments	♂	65·440	±·068	±1·062	±·048
	♀	65·365	±·066	±1·050	±·046

The values both of the means and of the standard deviations are exceedingly close in every case. Only for the standard deviation of total segments is the difference between the ♀ parents, weighted for ♂ and ♀ offspring respectively, greater than twice the P. E. σ. The average value of σ for the five characters in the case of the ♂s is ·994 and for the ♀s ·989. From this it may be fairly concluded that the production of an excess either of ♂ or of ♀ offspring is not associated with any difference of type in the ♀ parent.

Parental Correlations.

The parental correlations, like the fraternal, exhibit great diversity in value for the five characters chosen, varying after correction from ·215 for the half vertebrae up to ·411 for total segments. This last value, which is the highest of the series, is low in comparison with the value of parental heredity in other forms in which Pearson and Lee find that "its values lie between ·42 and ·52 and cluster round ·48" ('**03**, p. 379). The lowness of the average value for the five characters is yet more marked, being when corrected only ·307 (cf. Table 10), an exceedingly low value for parental heredity. Allusion has already been made (p. 333) to the possibility of some changes in the meristic series occurring during growth from the natal to the adult condition. Even if we suppose that such changes occur it is probable that they are fairly uniform, and as all the embryos examined were at approximately the same stage it is unlikely that the low values of the parental correlations are in any measure due to growth factors.

TABLE 10.

Parental Correlations.

	Raw value	Corrected value (a)
Anterior spine ...	·359 ± ·039	·378
Posterior spine ...	·196 ± ·043	·264
Whole vertebrae ...	·168 ± ·044	·215
Half vertebrae ...	·238 ± ·042	·270
Total segments ...	·416 ± ·037	·411
Mean	·275	·307

(a) The values given in the second column are corrected for selection of mothers out of adult ♀ population.

The low values of both the fraternal and parental correlations in *Spinax* are of great interest in connection with a recent contribution of Pearson, "On a Generalized Theory of Alternative Inheritance" ('**04**). Basing his calculations on the conception of gametic purity Pearson finds that the value of the parental correlation should be about ·3, that of the fraternal between ·3 and ·4. These theoretical values are considerably below the values hitherto deduced from actual observations ('**03**, pp. 379 and 387), and for this reason Pearson is inclined to pronounce against a theory of inheritance involving the conception of gametic purity. "The present investigation," he writes, "shows that in the theory of the pure gamete there is nothing in essential opposition to the broad features of linear regression, skew distribution, the geometric law of ancestral correlation, etc., of the biometric description of inheritance in populations. But it does show

that the generalized theory here dealt with is not elastic enough to account for the numerical values of the constants of heredity hitherto observed" ('04, p. 86). These theoretical values do however agree closely with the average values found for *Spinax*, and it must not be forgotten that we are here dealing with obviously discontinuous characters capable of simple numerical appreciation, and differing in nature, as far as we can at present judge, from any yet investigated. It may well be that the heredity of such characters is governed by Mendelian principles, though at present these principles are impossible of demonstration, owing to the existence of more than one pair of simple, or perhaps also of compound allelomorphs. To account for such families as that comprising embryos Nos. 260—270 is difficult except on an hypothesis of dominance, or on Pearson's hypothesis of "unit prepotency" ('03, p. 389). Here a family of 11 embryos of both sexes exhibits without exception extreme backward homoeosis, whilst the ♀ parent is characterized by marked forward homoeosis. It seems most natural to account for such a case by supposing that the unknown ♂ parent showed extreme backward homoeosis, and that this condition was dominant over that of marked forward homoeosis. The data are however too scanty and the possibilities too numerous to admit of profitable discussion on these points at present. More is doubtless to be learnt by selecting some simpler case in which the breeding can be easily controlled. This much new knowledge has at any rate been gained by the application of biometric methods, for the magnitude of the parental and fraternal correlations cannot but mean that the varying number of units in a primary linear meristic series does not depend alone on the individual environment, but that it is a character transmissible from one generation to another

IV. SUMMARY.

The chief results obtained from an examination of certain meristic characters in 567 specimens of *Spinax niger* may be briefly summarized as follows:—

(1) A well-marked sexual dimorphism permeates the meristic series, the ♂s showing a greater tendency to backward homoeosis than the ♀s.

(2) Equal numbers of ♂s and ♀s are born but the evidence derived from the numbers caught points to a preponderance of ♀s in the adult state.

(3) The variability of the ♂ embryos (as measured by the standard deviation) is markedly higher than that of the ♂ adults. In the case of the ♀s the variability is not very different in embryos and adults. This points to more stringent selection among the ♂s and accords well with the circumstance of their relative scarcity when adult.

(4) A comparison of the variability in different parts of the meristic series tells strongly against the hypotheses of vertebral intercalation or excalation; and this holds good either on the view that such processes occur uniformly, or that they

are brought about by random excision or interpolation of segments. The various positions of the different characters studied (e.g. spines &c.) are probably due to homoeosis, and the reality of this factor is brought out by a study of the correlations with one another of the various characters chosen.

(5) The study of the variations of the girdle canals supports the view that the different positions of the pelvic girdle are brought about by a process of homoeosis as opposed to one of excalation or intercalation.

(6) Certain discrepancies in the correlation values are most readily explicable on the supposition that some homoeotic changes take place between birth and the adult state. Probably only a few characters are affected, and notably the anterior spine.

(7) Although the ♂s exhibit backward homoeosis as compared with the ♀s there is no tendency for ♀ parents exhibiting marked backward homoeosis to produce more ♂ offspring, or for ♀s showing forward homoeosis to give rise to families having a larger proportion of ♀s.

(8) The values of the fraternal and parental correlations are much below what would be expected on the Law of Ancestral Heredity* and accord better with the theory of gametic purity for the characters studied. Nevertheless the values of these correlations are sufficiently large to prove that the number of units in a primary linear meristic series is not solely due to the individual environment but is a character transmitted from generation to generation.

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* [See footnote, p. 342. K. P.]

EXPLANATION OF PLATE.

Fig. 1, *a*—*c*. Showing the different positions of the anterior spine with regard to the vertebral cartilages. The numbers refer to the whole vertebra to which the spine is regarded as belonging. *bd* = basidorsal cartilage, *id* = interdorsal cartilage. 1*a* is taken from No. 146, 1*b* from 147 and 1*c* from 148 of the ♀ adults.

Fig. 2. Caudal end of embryo No. 139 × 8. The no. 30 indicates the 30th half vertebra.

Fig. 3. Caudal end of embryo No. 85 × 8. The 33rd and 34th half vertebrae are not separated as regards their centra.

Fig. 4. Longitudinal section through occipital region of skull and first few vertebrae. From ♀ adult No. 53. The spinal cord and brain have been removed.

Fig. 5. Preparation similar to Fig. 4. From ♀ adult No. 45. There is less condensation at the base of the skull than in the preceding figure.

Fig. 6, *a*—*c*. Longitudinal sections through the junction of half and whole vertebrae. The last whole vertebra is in each case numbered. 1*a* is taken from ♀ adult No. 64, 1*b* from ♀ adult No. 26, and 1*c* from ♀ adult No. 34.

Fig. 7. View of vertebral column of a single specimen after cleaning. In this specimen there are 42 whole vertebrae and 43 half vertebrae. The anterior spine is carried by vertebra 16, the posterior by vertebra 40. The dorsal spines in the caudal region have been omitted.

Note to Appendix, Tables 11—12.

These two tables present the raw data for adults of both sexes. Of the abbreviations used g. p. n. signifies the 1st girdle-piercing nerve, p. g. n. the post-girdle nerves, and coll. n. the number of nerves which take part in the formation of the *nervus collector*. Length w. v. and length h. v. denote the length of the series of whole and half vertebrae respectively and the values given are expressed in centimetres. When two numbers are given for the g. p. n. in a bracket as in individuals 9—11 of Table 11 it signifies that there are 2 g. p. n. and that both right and left sides are similar.

APPENDIX.

TABLE 11. ♂ *adults.*

Individual		*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*	*19*	*20*	*21*	*22*
G. p. n.	r.	29	28	29	28	29	29	28	29	28	27	28	29	28	29	26	28	29	29	27	28	28
	l.	,,	29-30	29	,,	,,	,,	,,	30	29	28	28-29	,,	29	,,	27	,,	,,	,,	28	,,	29
P. g. n.	r.	9	7	9	9	10	9	9	9	10	11	10	9	10	9	11	10	10	9	11	9	10
	l.	,,	8	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,
Coll. n.	r.	6	6	5	6	5	5	5	5	5	4	5	5	5	6	5	5	6	5	5	6	5
	l.	,,	,,	,,	,,	,,	,,	6	,,	,,	5	,,	,,	,,	,,	,,	6	,,	,,	,,	,,	,,
Ant. sp.		—	—	16	16	16-17	17	16	16	16	16	16	18	16	16	16	16	16	16	16	15	15
Post. sp.		39	39	40	40	40-41	42	41	40	41	40	40	41	41	41	39	40-41	41	41	40	41	40
W. vert.		45	45	44	45	45	44	45	44	45	44	42	44	45	45	45	43	44	45	45	44	46	45
H. vert.		37	39	42	38	38	44	36	40	40	43	43	41	40	38	40	40	42	40	38	37	37	40
Total segs.		64	65	65	64	64	66	63	64	65	66	64	65	65	64	65	63	65	65	64	63	65	65

Individual		*23*	*24*	*25*	*26*	*27*	*28*	*29*	*30*	*31*	*32*	*33*	*34*	*35*	*36*	*37*	*38*	*39*	*40*	*41*	*42*	*43*
G. p. n.	r.	28	28	27-28	29	29	29	29	30-31	28	28-29	27	30	28	29	28	29	28	28	28	30	28
	l.	29	29	28-29	30	30	30	28-29	30	29	29	28	31	,,	28-29	,,	,,	,,	29	,,	,,	,,
P. g. n.	r.	9	10	11	9	9	9	9	9	10	10	10	9	9	9	9	8	10	11	9	9	9
	l.	,,	,,	10	,,	,,	,,	10	,,	,,	9	,,	,,	,,	10	,,	,,	,,	,,	,,	,,	,,
Coll. n.	r.	5	5	5	5	6	6	6	6	5	5	5	6	6	6	5	6	5	5	4	5	5
	l.	,,	,,	6	,,	,,	,,	5	,,	6	,,	,,	,,	,,	5	,,	,,	,,	,,	,,	,,	,,
Ant. sp.		15	15	15-16	16	17	16-17	16	16	15	16	16	18	16	16	15	16	16	17	15	16	15
Post. sp.		39	39	41	42	41	42	40	42	40	40	40	42	40	41	40	41	40	40	39	41	39
W. vert.		43	43	45	47	45	46	43	46	45	45	44	47	44	45	43	46	44	45	44	45	44
H. vert.		40	41	39	40	38	38	43	42	41	40	40	38	38	40	42	39	42	41	41	42	39
Total segs.		63	64	65	67	64	65	65	67	66	65	64	66	63	65	64	66	65	66	65	66	64

Individual		*44*	*45*	*46*	*47*	*48*	*49*	*50*	*51*	*52*	*53*	*54*	*55*	*56*	*57*	*58*	*59*	*60*	*61*	*62*	*63*
G. p. n.	r.	29	27	30	28	29	28	29	30	29	28-29	28	29	29	28	29	29	27	29-30	30-31	29
	l.	,,	,,	31	,,	30	29	,,	,,	28-29	29	29	,,	,,	29	30	,,	28	29	31	,,
P. g. n.	r.	9	10	8	10	10	9	8	9	9	8	9	9	9	9	9	8	10	9	9	9
	l.	,,	,,	,,	,,	,,	,,	,,	,,	10	,,	,,	,,	,,	,,	,,	,,	,,	,,	8	,,
Coll. n.	r.	5	5	5	5	4	5	6	6	5	5	5	5	5	5	6	6	4	4	5	6
	l.	,,	4	,,	,,	,,	,,	,,	5	4	,,	,,	,,	6	,,	,,	,,	,,	,,	6	,,
Ant. sp.		16	16	16	16	17	15	16	15-16	16-17	16	16	16	16	16	16	16	15	16	17	15
Post. sp.		41	39-40	41	40	41	40	40	41	40-41	40	41	41-42	41	42	41	41	40	41	42-43	40
W. vert.		45	43	45	44	45	44	44	46	46	43	44	46	45	46	46	45	44	45	48	45
H. vert.		40	43	42	41	38	41	40	38	37	43	44	42	40	41	41	41	39	42	37	41
Total segs.		65	65	66	65	64	65	64	65	65	66	67	65	67	67	67	66	64	66	67	66
Length w. v.		—	—	18·6	18·3	19·7	19·8	19·9	16·2	19·6	21·2	19·5	19·9	16·2	20·5	18·8	16·5	17·4	16·7	19·1	15·2
Length h. v.		—	—	10·1	10·4	10·1	11·0	10·2	9·2	9·1	12·1	11·7	10·5	9·2	11·0	10·0	8·8	9·6	9·9	9·5	8·3
Total length		—	—	28·7	28·7	29·8	30·8	30·1	25·4	28·7	23·3	31·2	30·4	25·4	31·5	28·8	25·3	27·0	26·6	28·6	23·5

Individual		*64*	*65*	*66*	*67*	*68*	*69*	*70*	*71*	*72*	*73*	*74*	*75*	*76*	*77*	*78*	*79*	*80*	*81*	*82*
G. p. n.	r.	29	29	30	31	28	29	28	28	29	28	27	28-29	28	29	29	28	28	29	29-30
	l.	29-30	28-29	31	,,	29	,,	,,	,,	,,	29	28	28	,,	,,	,,	29	,,	,,	29
P. g. n.	r.	9	9	9	8	9	9	10	9	10	9	10	9	9	10	10	10	9	9	9
	l.	8	10	8	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	9	,,	,,	,,	,,	,,
Coll. n.	r.	6	6	6	6	5	5	5	5	6	5	4	5	6	5	5	5	5	6	5
	l.	,,	5	7	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	6	,,	,,	,,	,,	6
Ant. sp.		15	15-16	16	17	15	17	16	16	16	16	15	15-16	15	15	16	,,	,,	,,	16-17	16	16
Post. sp.		40	41	43	43	40	41	40	41	39	40	40	40	40	41	41	40-41	40	39	40-41		
W. vert.		44	45	47	48	45	44	43	43	44	45	44	43	44	46	46	45	43	43	44		
H. vert.		41	40	40	39	40	41	45	44	39	40	41	43	39	39	40	40	42	41	41		
Total segs.		65	65	67	68	65	65	66	65	64	65	65	65	64	66	66	65	64	64	65		
Length w. v.		17·0	20·2	20·2	19·7	20·9	20·0	19·2	14·0	15·1	20·4	19·5	19·0	19·5	20·2	18·3	18·2	17·4	16·2	19·3		
Length h. v.		8·9	10·3	10·3	9·6	9·1	10·8	11·3	9·1	8·5	10·3	10·4	10·9	10·3	10·4	10·1	9·8	10·6	9·3	10·2		
Total length		25·9	30·5	30·5	30·3	30·0	30·8	30·5	23·1	23·6	30·7	29·9	29·9	29·8	30·6	28·4	28·0	28·0	25·5	29·5		

Individual		*83*	*84*	*85*	*86*	*87*	*88*	*89*	*90*	*91*	*92*	*93*	*94*	*95*	*96*	*97*	*98*	*99*	*100*
G. p. n.	r.	28	29	29-30	29-30	28	28	27	28-29	28	29	27	28	28	28-29	29	29	28	27
	l.	29	,,	,,	,,	29	29	28	28	,,	,,	,,	29	,,	29	,,	30	,,	,,
P. g. n.	r.	10	10	9	9	10	9	10	10	9	9	11	10	10	10	9	8	10	11
	l.	,,	,,	10	10	,,	,,	,,	,,	,,	,,	,,	,,	,,	9	,,	,,	,,	,,
Coll. n.	r.	5	6	6	6	5	5	4	5	5	6	4	4	5	5	5	6	5	4
	l.	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	6	,,	5	,,	,,
Ant. sp.		15	17	16	16	17	15	15	15-16	16	17	15	16	16-17	16	16	16	16	16
Post. sp.		40	41	41	41-42	40	40	39	41	40	41-42	40	41	39-40	41	40	40	40	40
W. vert.		44	44	46	46	44	44	43	44	45	46	43	44	44	44	45	43	44	46
H. vert.		41	42	40	42	40	39	40	43	38	39	43	42	40	45	39	43	41	37
Total segs.		65	65	66	67	64	64	63	66	64	66	65	65	64	67	65	65	65	65
Length w. v.		20·2	15·8	19·8	19·8	17·8	19·6	18·4	17·6	20·7	18·8	18·2	18·8	19·2	17·8	16·7	18·9	20·0	19·7
Length h. v.		10·7	9·5	10·1	10·8	9·3	10·6	10·4	10·4	10·2	10·2	9·9	11·4	10·3	10·9	9·7	11·6	11·4	8·9
Total length		30·9	25·3	29·9	30·6	27·1	30·2	28·8	28·0	30·9	29·0	28·1	30·2	29·5	28·7	26·4	30·5	31·4	28·6

TABLE 12.

♀ *adults.*

Individual	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*	*19*	*20*	*21*	*22*
G. p. n. r.	30	29	29	30	28	28	28-29	28	30	30	29	29	29-30	29-30	30-31	30	30	29	29	30	29	31
G. p. n. l.	31	,,	,,	31	29	29	29	29	,,	,,	,,	,,	29	28-29	30	,,	,,	30	30	31	30	,,
P. g. n. r.	8	7	7	8	9	9	8	9	6	8	7	8	7	7	8	8	8	9	8	8	8	6
P. g. n. l.	,,	,,	,,	,,	,,	,,	7	,,	,,	,,	,,	,,	,,	8	,,	,,	,,	8	,,	,,	7	,,
Coll. n. r.	6	6	6	6	4	4	5	5	6	5	6	5	6	6	5	6	6	6	5	6	6	6
Coll. n. l.	,,	,,	5	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	5	,,	,,	,,	7	,,	,,	,,	,,
Ant. sp.	—	—	—	—	—	—	—	—	—	16	16	16-17	17-18	16-17	17	17	17	16	17	17-18	16	16-17
Post. sp.	—	—	—	—	42	41-42	41	42	42	42	41	41-42	41-42	41	42-43	41	43	41-42	42	42-43	41	42-43
W. vert.	47	44	45	46	45	46	45	45	44	46	45	46	45	44	46	45	47	45	45	47	45	47
H. vert.	39	41	38	41	46	39	39	41	43	38	40	39	41	42	42	40	42	43	41	38	39	39
Total segs.	67	65	64	67	68	66	65	66	66	65	65	66	66	65	67	65	68	67	66	66	65	67

Individual	*23*	*24*	*25*	*26*	*27*	*28*	*29*	*30*	*31*	*32*	*33*	*34*	*35*	*36*	*37*	*38*	*39*	*40*	*41*
G. p. n. r.	29	30	30	29	30	30	28	29	28	29	29	29	28	29	29	28	28-29-30	29	29
G. p. n. l.	,,	,,	,,	,,	29-30	,,	29	,,	29	,,	,,	,,	29	30	30	,,	,,	30	30
P. g. n. r.	8	7	8	7	8	8	9	8	9	8	7	8	8	8	9	9	9	8	8
P. g. n. l.	7	8	,,	,,	,,	,,	8	,,	,,	,,	8	,,	,,	,,	,,	,,	,,	,,	,,
Coll. n. r.	6	6	5	6	5	5	6	6	5	5	5	6	5	5	5	5	4	5	5
Coll. n. l.	,,	,,	,,	,,	4	,,	,,	,,	,,	6	,,	,,	4	,,	,,	,,	5	6	,,
Ant. sp.	16	16	17	17	16	17	16	16-17	16	17	16	16	16	15-16	16	16	16-17	16	16
Post. sp.	40-41	41	41-42	40-41	41	41	40	40	41	41	40	42	40	41	42	40	41	42	41
W. vert.	43	46	45	44	46	44	44	44	44	44	44	46	43	44	45	44	46	47	44
H. vert.	42	37	41	41	37	40	39	40	42	40	41	41	41	39	43	39	39	37	42
Total segs.	64	65	66	65	65	64	64	64	65	64	65	67	64	64	67	64	66	66	65

Individual	*42*	*43*	*44*	*45*	*46*	*47*	*48*	*49*	*50*	*51*	*52*	*53*	*54*	*55*	*56*	*57*	*58*	*59*	*60*
G. p. n. r.	30	29	30	29	28-29	30	29	28	29	29	29	30-31	29	30	30	28	30	31	29
G. p. n. l.	29-30	30	31	30	29	,,	30	,,	30	,,	,,	30	,,	,,	29	29	,,	,,	,,
P. g. n. r.	7	8	8	8	10	7	8	8	8	8	7	8	8	7	7	8	7	8	8
P. g. n. l.	8	,,	,,	,,	9	,,	,,	,,	,,	,,	8	,,	,,	,,	8	,,	,,	7	,,
Coll. n. r.	6	5	6	6	4	5	6	5	5	5	5	6	5	5	7	5	6	6	5
Coll. n. l.	5	6	,,	,,	5	,,	,,	6	,,	,,	6	,,	,,	,,	6	,,	,,	,,	6
Ant. sp.	16	16-17	15-16	17	16	15-16	15	16	16	17	15	—	—	—	—	—	—	—	—
Post. sp.	41-42	42	41-42	42	41	40-41	41	40	42	41	41	43-44	41	42	42	42	42	44	39
W. vert.	47	45	45	48	45	46	44	44	46	45	45	48	44	45	45	46	47	48	44
H. vert.	38	41	42	37	42	37	42	39	36	40	37	39	42	40	40	37	38	40	39
Total segs.	66	66	66	67	66	65	65	64	64	65	64	68	65	65	65	65	66	68	64
Fin rays	—	—	—	—	—	—	—	—	—	—	—	18	17	18	18	17	18	20	16

Individual	*61*	*62*	*63*	*64*	*65*	*66*	*67*	*68*	*69*	*70*	*71*	*72*	*73*	*74*	*75*	*76*	*77*	*78*	*79*	*80*	*81*
G. p. n. r.	29	27	30	28	30	29	29	29	29	28-29	29	29	29	28	29	29	29-30	30	29-30	28-29	28-29
G. p. n. l.	,,	28	,,	,,	31	,,	30	,,	,,	29	30	,,	30	29	,,	,,	30	31	30	29	29
P. g. n. r.	8	10	7	9	8	8	9	8	8	9	9	7	9	8	8	8	8	7	9	9	10
P. g. n. l.	,,	,,	,,	,,	,,	,,	,,	,,	,,	8	,,	,,	,,	,,	,,	,,	7	,,	8	8	8
Coll. n. r.	5	5	5	6	5	6	5	5	5	4	5	6	6	5	6	5	5	6	5	5	4
Coll. n. l.	,,	,,	,,	,,	,,	,,	,,	,,	,,	5	6	,,	5	,,	,,	,,	6	,,	,,	,,	5
Ant. sp.	—	15	17-18	16-17	16	16	15-16	15	16	15	16	16	16	16	15-16	16	16	16	17	16	16
Post. sp.	41	41	42	41	42	41-42	40	40	41-42	39-40	42	41	41-42	40	40	40	42	42	42	41	41
W. vert.	45	45	46	46	47	44	45	43	45	44	44	45	46	44	45	44	45	46	46	45	44
H. vert.	40	39	42	40	38	42	42	43	40	40	41	41	36	38	39	40	43	41	38	40	42
Total segs.	65	65	67	66	66	65	66	65	65	64	65	66	64	63	65	64	67	67	65	65	65
Fin rays	17	18	16	17	18	16	18	17	17	18	19	15	15	16	19	17	19	15	18	17	18

TABLE 12—(*continued*).

Individual	*82*	*83*	*84*	*85*	*86*	*87*	*88*	*89*	*90*	*91*	*92*	*93*	*94*	*95*	*96*	*97*	*98*	*99*	*100*	*101*	*102*	*103*	*104*
G. p. n. r.	30	28	30	29	30	29	29-30	28	30	29	29	28	29	28	29	29	28	29	29	30-31	29	29	28
G. p. n. l.	,,	29	31	,,	,,	,,	30	29	,,	,,	,,	,,	,,	,,	30	,,	,,	,,	,,	30	,,	,,	,,
P. g. n. r.	7	9	7	8	7	8	8	8	6	8	7	8	8	9	8	7	9	8	8	7	7	8	8
P. g. n. l.	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	8	,,	9	,,	,,	,,	,,	,,	,,	8	,,	,,	,,
Coll. n. r.	6	5	5	5	6	5	4	5	6	5	5	5	5	5	5	5	6	5	5	6	5	5	5
Coll. n. l.	,,	,,	,,	,,	,,	,,	6	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	5	6	6	,,
Ant. sp.	16-17	15	16	16	16	16-17	15-16	16	16	15-16	15-16	15	17	15	16	16	14	17	16	17	16	15-16	17
Post. sp.	42	40	42	40-41	41	41-42	41	40	41	41	41	39	41	41	41	40-41	39	42	40	41	40	41	40
W. vert.	45	43	46	44	46	46	45	45	46	44	44	44	45	46	44	44	44	43	44	45	44	46	44
H. vert.	41	45	39	42	40	39	40	39	38	42	42	40	41	39	41	44	41	48	41	42	43	38	42
Total segs.	66	66	66	65	66	66	65	65	65	65	65	64	66	66	65	66	65	67	65	66	66	65	65
Fin rays	17	17	17	18	15	16	17	17	17	16	16	15	16	19	17	17	19	16	16	16	18	19	18

Individual	*105*	*106*	*107*	*108*	*109*	*110*	*111*	*112*	*113*	*114*	*115*	*116*	*117*	*118*	*119*	*120*	*121*	*122*	*123*	*124*	*125*
G. p. n. r.	28	30	30	28	30	29	29	29	29	29	30	30	28	27	29	29	29	29	30	29-30	28
G. p. n. l.	29	,,	31	,,	30-31	,,	30	,,	,,	,,	29-30	,,	,,	,,	30	,,	28-29	30	29-30	30	,,
P. g. n. r.	10	7	7	8	7	8	8	7	8	7	9	8	9	8	8	8	7	8	7	8	8
P. g. n. l.	,,	,,	,,	,,	,,	,,	7	,,	,,	8	,,	7	,,	,,	,,	,,	8	,,	8	7	,,
Coll. n. r.	5	5	5	5	6	5	5	5	6	5	6	5	5	5	5	5	5	5	5	4	6
Coll. n. l.	,,	6	,,	,,	,,	,,	,,	,,	,,	,,	5	,,	4	,,	,,	,,	4	,,	4	5	,,
Ant. sp.	15	16	16-17	15	14	16	16	16	16	16	17	17	17	16	16	16	16	16	16	17	15
Post. sp.	40	42	41-42	40	41	41	42	40	41	40	42	42	41	41	41	41	39	41	41	41	40-41
W. vert.	45	45	45	45	45	44	45	45	45	46	45	46	45	44	46	45	44	45	44	45	46
H. vert.	38	43	41	39	39	41	41	40	40	37	43	39	41	42	37	39	40	41	42	40	37
Total segs.	64	67	66	65	65	65	66	65	65	65	67	66	66	65	65	65	64	66	65	65	65
Fin rays	17	16	17	17	16	15	16	18	17	20	17	17	18	17	17	16	17	18	18	17	17
Length w. v.	21·0	19·3	18·2	19·7	23·0	22·9	20·5	20·0	21·7	20·2	24·6	21·2	16·0	21·1	22·2	22·0	20·2	22·3	22·5	23·5	19·9
Length h. v.	10·5	10·7	10·7	10·1	11·2	12·0	11·2	10·5	11·0	9·7	12·4	10·5	9·1	11·7	10·6	10·4	10·9	11·4	11·2	12·0	9·9
Total length	31·5	30·2	28·9	29·8	34·2	34·9	31·7	30·5	32·7	29·9	37·0	31·7	25·1	32·8	32·8	32·4	31·1	33·7	33·7	35·5	29·8

Individual	*126*	*127*	*128*	*129*	*130*	*131*	*132*	*133*	*134*	*135*	*136*	*137*	*138*	*139*	*140*	*141*	*142*	*143*	*144*	*145*	*146*	*147*	*148*
G. p. n. r.	30	29	30	29-30	29	29-30	28	29	27-28	30	29	28	28	30	29	28	29	30	29-30	29	29	29	29
G. p. n. l.	31	30	,,	30	30	30	29	30	27	,,	,,	,,	29	,,	,,	,,	,,	31	29	,,	,,	,,	,,
P. g. n. r.	8	8	7	9	8	7	9	8	9	8	9	9	8	7	8	8	7	8	8	7	7	8	8
P. g. n. l.	9	,,	,,	8	,,	6	7	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	,,	9
Coll. n. r.	6	5	6	5	5	5	4	5	5	6	5	5	5	5	5	6	5	5	5	6	6	5	5
Coll. n. l.	5	,,	,,	6	,,	6	5	,,	,,	,,	,,	,,	,,	,,	,,	5	,,	,,	,,	,,	,,	,,	,,
Ant. sp.	16-17	17	17	16	16	16	16	16	15	15	16	15	15	16	15	15	16	16	16	16	16	16	17
Post. sp.	42	41	41	42	42	42	40	42	40	41	42	40	40	41	41	39	41	42	41	41	40	41	42
W. vert.	47	46	46	47	47	45	44	46	44	45	45	44	43	45	45	43	45	47	46	45	44	44	45
H. vert.	38	39	38	38	40	43	41	41	40	43	42	41	41	43	43	41	40	40	40	40	42	42	42
Total segs.	66	66	65	66	67	67	65	67	64	67	66	65	64	67	67	64	65	67	66	65	65	65	66
Fin rays	17	18	18	—	17	17	18	18	16	18	17	16	16	17	19	18	—	17	16	18	16	19	17
Length w. v.	19·7	22·6	24·8	21·2	22·9	20·6	20·1	19·1	19·7	22·8	21·0	19·9	19·9	18·1	17·4	22·4	20·7	17·3	19·8	19·4	23·8	22·1	22·4
Length h. v.	10·3	10·7	11·8	9·8	10·8	10·2	11·1	10·2	10·4	11·8	10·9	11·1	11·9	10·2	10·2	11·6	11·5	9·0	10·9	11·0	11·8	12·2	11·9
Total length	30·0	33·3	36·6	31·0	33·7	30·8	31·2	29·3	30·1	34·6	31·9	31·0	31·8	28·3	27·6	34·0	32·2	26·3	30·7	30·4	35·6	34·3	34·3

Individual	*149*	*150*	*151*	*152*	*153*	*154*	*155*	*156*	*157*	*158*	*159*	*160*	*161*	*162*	*163*
Ant. sp.	15	15	16	16	16-17	16	16-17	17	17	17	17	16	16	16-17	16
Post. sp.	40	40	41	41	40	41	42	41	41	41	42	42	41	41	41-42
W. vert.	44	44	45	45	44	45	46	46	45	45	46	47	45	44	46
H. vert.	38	41	37	39	42	41	41	37	39	41	40	41	38	43	42
Total segs.	63	65	64	65	65	66	67	65	65	66	66	68	64	66	67

TABLE 13.

Embryos, ♂ and ♀.

Individual	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28
Sex	♀	♂	♀	♀	♀	♀	♀	♀	♀	♀	♀	♂	♂	♂	♂	♀	♀	♂	♂	♀	♂	♂	♂	♀	♂	♀	♀	♂
Ant. sp.	16	16	16	16	15	16	16	16	16	16	17	15	15	16	18	17	16	15	16	15	16	15	16	17	16	16	16	15-16
Post. sp.	41	39	39	40	40	40	39	40	41	41	42	40-41	40-41	40	42	41	41	40	42	41	41	40	40	42	41	40	41	40
W. vert.	45	43	42	45	43	44	43	44	45	46	45	45	44	44	44	45	45	44	47	44	45	44	44	46	43	44	44	44
H. vert.	38	39	40	38	40	41	41	43	40	38	44	40	41	40	45	36	36	38	38	40	42	40	38	40	42	40	40	39
Total segs.	64	63	62	64	63	65	64	66	65	65	67	65	65	64	67	64	63	63	66	64	66	64	63	66	64	64	64	64

Individual	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56
Sex	♂	♂	♀	♂	♂	♂	♀	♂	♂	♀	♀	♀	♂	♀	♂	♂	♂	♀	♂	♂	♀	♀	♀	♀	♂	♂	♂	♀
Ant. sp.	16	16	17	15-16	16	17	16	17	16	16	17	16	16	16	16	17	15	15	16	16	16	16	16	17	15	16	16	16
Post. sp.	42	41-42	41	40	42	42	41	42	41	41-42	42-43	43	41	40	41-42	41	40	40	43	39	41	42	42	40	40	40-41	39	40
W. vert.	45	46	44	45	47	46	44	47	45	46	47	47	46	44	46	45	44	43	51	43	45	47	48	43	43	44	43	43
H. vert.	38	38	40	38	38	38	40	36	40	42	38	38	38	40	40	41	38	42	32	38	38	38	36	41	42	39	40	42
Total segs.	64	65	64	64	66	65	64	65	65	67	66	66	66	65	64	66	63	64	67	62	64	66	66	64	64	64	63	64

Individual	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84
Sex	♀	♂	♂	♀	♀	♀	♀	♀	♂	♂	♀	♀	♂	♂	♀	♀	♀	♂	♂	♂	♂	♀	♀	♀	♀	♀	♂	♂
Ant. sp.	16	15-16	15-16	16	16	16	16	16	15	16	16	16	16	16	17	16	16	15	15-16	16	16	15	15	16	16	16	16	16
Post. sp.	41	39-40	40	41	41	41	40	41	41	40	41	41-42	40	40	41	38	41	40-41	39-40	40-41	40-41	40	40	40-41	41	40	41	41
W. vert.	44	42	43	45	44	45	44	44	46	43	44	47	44	45	44	43	47	44	44	45	44	43	43	44	44	43	45	45
H. vert.	41	42	40	38	41	39	42	40	37	40	39	35	39	39	38	40	37	40	38	39	39	42	40	40	41	41	37	37
Total segs.	65	63	63	64	65	65	65	64	65	63	64	65	64	65	63	63	66	64	63	65	64	64	63	64	65	64	64	64

Individual	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	101	102	103	104	105	106	107	108	109	110	111	112
Sex	♂	♂	♂	♂	♂	♀	♀	♀	♂	♂	♂	♀	♂	♀	♂	♂	♂	♀	♂	♀	♂	♀	♂	♀	♀	♀	♀	♂
Ant. sp.	16	16	16	16	15	16	16	16	16	16-17	16-17	17	16	16	16-17	16-17	16	16	16	16	16	15-16	15-16	16	16	16	16	16
Post. sp.	41	40	40	40	40	41-42	42	40	41	40	41	41	40	42	42-43	41-42	41	39-40	40-41	41-42	40-41	40	40	40	41-42	40	41	40
W. vert.	45	44	45	45	45	45	46	44	45	45	45	46	43	46	46	45	45	44	44	45	44	43	43	44	44	44	44	44
H. vert.	41	38	39	37	39	40	41	40	41	41	39	38	41	38	42	41	40	41	41	42	40	43	42	40	44	41	42	40
Total segs.	66	63	65	64	65	65	67	64	66	66	65	65	64	65	67	66	65	65	65	66	64	65	64	64	66	65	65	64

Individual	113	114	115	116	117	118	119	120	121	122	123	124	125	126	127	128	129	130	131	132	133	134	135	136	137	138	139	140
Sex	♂	♀	♀	♀	♀	♀	♀	♂	♂	♂	♂	♀	♀	♂	♂	♂	♂	♂	♀	♀	♂	♀	♀	♀	♀	♂	♂	♀
Ant. sp.	16	15-16	15-16	15-16	16	16	15	16	15	15-16	15	15	16	16	16	16	16	16	16	16	16	16	16	15	16	16	16	15
Post. sp.	39	40-41	41	41-42	42	41	40	41	41	40	39-40	40	41	40	41	41	40	41	41	40	40	40	40-41	40	40	39	40	40
W. vert.	43	45	46	46	46	45	44	46	46	44	44	44	45	44	46	45	44	45	45	44	44	43	44	43	44	43	43	44
H. vert.	43	38	37	37	39	39	40	38	39	40	40	42	39	41	39	40	40	40	40	40	39	41	41	40	38	40	40	41
Total segs.	65	64	65	65	66	65	64	65	66	64	64	65	65	65	66	65	64	65	65	64	64	64	65	63	63	63	63	65

Individual	141	142	143	144	145	146	147	148	149	150	151	152	153	154	155	156	157	158	159	160	161	162	163	164	165	166	167	168
Sex	♀	♀	♀	♀	♀	♂	♂	♂	♂	♂	♂	♂	♂	♂	♂	♂	♂	♂	♀	♀	♀	♀	♂	♀	♀	♀	♀	♀
Ant. sp.	16	16	16	16	15-16	16	16	16	16	16	16	16	16	16	16	16	16	16	16	16	16	16	15	16	16	16	16	16
Post. sp.	41	41-42	41	42	41	40-41	40	41	41-42	40-41	40	41	40	40	41-42	40	40	41	41	41	40	40	40	40	40	40-41	40	40
W. vert.	45	46	45	45	46	45	44	45	46	45	44	46	44	41	47	45	45	46	45	45	44	45	42	44	44	45	44	45
H. vert.	39	38	39	40	38	40	40	42	40	40	42	39	42	42	39	40	40	39	42	40	41	40	44	39	41	40	40	39
Total segs.	65	65	65	65	65	65	64	66	66	65	65	66	65	65	67	65	65	66	66	65	65	65	64	64	65	65	64	65

TABLE 13—(*continued*).

Individual	169	170	171	172	173	174	175	176	177	178	179	180	181	182	183	184	185	186	187	188	189	190	191	192	193	194
Sex	♀	♀	♀	♂	♂	♂	♂	♀	♀	♂	♂	♀	♂	♀	♀	♀	♀	♂	♀	♀	♂	♂	♀	♂	♀	♂
Ant. sp.	16	16	16	16	16	16	16	15	15	15	15	16	16	15	16	17	16	16	16	17	17	16	16	16	16	17
Post. sp.	40	41	42	41	40	41	40	40	41	40	41	42	39	40	40	41	41	39	40	41	41	40	40	41	40	40
W. vert.	43	46	45	45	44	46	43	44	46	44	44	45	44	45	44	46	44	43	46	45	45	44	46	45	45	45
H. vert.	44	38	40	39	41	37	42	41	39	41	43	40	40	39	39	40	43	43	38	41	40	40	37	40	38	40
Total segs.	65	65	65	65	65	65	64	65	66	65	66	65	64	65	64	66	66	65	65	66	65	64	65	65	64	65

Individual	195	196	197	198	199	200	201	202	203	204	205	206	207	208	209	210	211	212	213	214	215	216	217	218	219	220
Sex	♂	♂	♀	♀	♂	♂	♂	♂	♂	♀	♀	♀	♂	♀	♂	♀	♀	♀	♂	♀	♂	♂	♀	♂	♀	♀
Ant. sp.	16	16	16	17	16	17	16	15-16	15	15	15	16	16	16	15	15	17	16	16	16	16	16	16	16	17	17
Post. sp.	40	40	41	41	40	42	41	40-41	40	40-41	41	40	40	40	40	39	42	42	41	41	42	41	41	40	42	42
W. vert.	45	45	46	45	45	47	46	45	44	45	45	45	45	45	44	43	46	45	45	46	46	46	44	43	46	47
H. vert.	40	39	38	42	41	36	39	39	41	39	39	38	39	41	38	40	40	43	43	41	42	39	42	43	41	40
Total segs.	65	65	65	66	66	65	66	65	65	65	65	64	65	66	63	63	66	67	67	67	67	66	65	65	67	67

Individual	221	222	223	224	225	226	227	228	229	230	231	232	233	234	235	236	237	238	239	240	241	242	243	244	245	246
Sex	♂	♀	♂	♂	♀	♂	♀	♀	♂	♂	♀	♀	♀	♀	♂	♂	♂	♀	♂	♀	♂	♀	♂	♀	♀	♀
Ant. sp.	17	17	16	16	17	16	17	17	16	18	15-16	17	16-17	17	16	17	15	16	15	17	16	17	17	17	16	17
Post. sp.	41	42	40	40-41	40	40	41	41	40	42	39	41	41	40	39	39	39	39	39	40	40	40	40	40	40	40
W. vert.	45	46	44	44	43	43	46	46	45	46	45	45	45	44	44	44	43	44	44	44	44	44	43	44	44	45
H. vert.	42	41	38	40	40	43	39	39	36	38	39	37	38	39	40	42	41	40	38	39	43	41	40	42	41	40
Total segs.	66	67	63	64	63	65	66	66	63	65	65	64	64	64	64	65	64	64	63	64	66	65	63	65	65	65

Individual	247	248	249	250	251	252	253	254	255	256	257	258	259	260	261	262	263	264	265	266	267	268	269	270	271	272
Sex	♀	♀	♂	♀	♀	♂	♀	♀	♀	♀	♂	♀	♂	♀	♂	♀	♂	♂	♂	♀	♂	♂	♀	♀	♂	♀
Ant. sp.	17	17	17	15	16	15	15	17	16	16	16	16	16	16	17	15	16	16	16	16	16	16	16	16	16	15
Post. sp.	41	41	41	39	40	40	40	41	40	40	40	42	40	40	39	39	39	39	40	39	39	39	40	40	42	42
W. vert.	46	45	46	44	45	44	44	45	44	44	44	46	44	43	42	43	42	42	42	43	42	41	44	44	47	47
H. vert.	41	40	39	41	39	40	42	41	43	40	40	40	40	39	42	42	44	41	44	42	41	46	40	40	42	40
Total segs.	67	65	66	65	65	64	65	66	66	64	64	66	64	63	63	64	64	63	64	64	63	64	64	64	68	67

Individual	273	274	275	276	277	278	279	280	281	282	283	284	285	286	287	288	289	290	291	292	293	294	295	296	297	298
Sex	♂	♀	♂	♂	♀	♀	♂	♂	♀	♂	♀	♀	♀	♀	♀	♂	♀	♀	♀	♀	♂	♀	♂	♂	♂	♀
Ant. sp.	16	16	15	16	16	16	15	15-16	16	16	18	16	16	16	17	16	15	15	15	16	16	16	15	16	15	16
Post. sp.	40	41	42	42	42	41	42	39	40	40	41	40	41	40	41	41	40	41	40	40	39	41	41	40	40	41
W. vert.	45	45	46	47	46	45	46	43	45	44	46	46	46	45	46	45	45	45	44	43	44	44	45	44	44	45
H. vert.	39	44	44	40	40	42	40	40	37	39	40	38	40	38	39	41	38	40	41	41	40	41	39	40	40	42
Total segs.	65	67	68	67	66	66	66	63	64	64	66	65	66	64	66	66	64	65	65	64	64	65	65	64	64	66

Individual	299	300	301	302	303	304
Sex	♀	♂	♀	♂	♀	♂
Ant. sp.	16	15	16	16	16	16
Post. sp.	41	40	41	40	41	41
W. vert.	45	44	45	44	45	45
H. vert.	43	42	40	41	41	42
Total segs.	67	65	65	65	66	66

75–82	from ♀	*84*	*149–162*	from ♀	*86*	*211–222*	from ♀	*155*
83–92	,, ,,	*85*	*163–164*	,, ,,	(?)	*223–234*	,, ,,	*156*
93–105	,, ,,	*87*	*165–168*	,, ,,	(?)	*235–246*	,, ,,	*157*
106–107	,, ,,	*88*	*169–174*	,, ,,	*149*	*247–259*	,, ,,	*158*
108–113	,, ,,	*89*	*175–180*	,, ,,	*150*	*260–270*	,, ,,	*159*
114–123	,, ,,	*90*	*181–187*	,, ,,	*151*	*271–279*	,, ,,	*160*
124–132	,, ,,	*91*	*188–193*	,, ,,	*152*	*280–288*	,, ,,	*161*
133–139	,, ,,	*93*	*194–201*	,, ,,	*153*	*289–296*	,, ,,	*162*
140–148	,, ,,	*92*	*202–210*	,, ,,	*154*	*297–304*	,, ,,	*163*

TABLE 14.

Anterior spine	*14*	*15*	*16*	*17*	*18*	No.
♂ adults	—	21	60	15	2	*98*
♂ embryos	—	24	104	15	2	*145*
♀ adults	2	19	81	40	3	*145*
♀ embryos	—	22	107	29	1	*159*

		♂ adults	♂ embryos	♀ adults	♀ embryos
M.		15·979	15·965	16·159	16·056
P. E. M.	±	·046	·031	·040	·031
σ		·6697	·5544	·7208	·5854
P. E. σ	±	·0323	·0219	·0286	·0221
C. V.		4·19	3·47	4·46	3·65

TABLE 15.

Posterior spine	*38*	*39*	*40*	*41*	*42*	*43*	*44*	No.
♂ adults	—	10	41	37	9	3	—	*100*
♂ embryos	—	18	62	45	18	2	—	*145*
♀ adults	—	5	29	67	52	4	2	*159*
♀ embryos	1	8	60	63	25	2	—	*159*

		♂ adults	♂ embryos	♀ adults	♀ embryos
M.		40·540	40·476	41·170	40·686
P. E. M.	±	·060	·051	·048	·040
σ		·8879	·9103	·9057	·8625
P. E. σ	±	·0423	·0361	·0343	·0281
C. V.		2·19	2·24	2·20	2·12

TABLE 16.

Whole Vertebrae	*41*	*42*	*43*	*44*	*45*	*46*	*47*	*48*	*49*	*50*	*51*	No.
♂ adults	—	1	15	32	32	15	3	2	—	—	—	*100*
♂ embryos	1	7	19	48	42	20	7	—	—	—	1	*145*
♀ adults	—	—	7	46	59	35	13	3	—	—	—	*163*
♀ embryos	—	1	19	50	51	30	7	1	—	—	—	*159*

		♂ adults	♂ embryos	♀ adults	♀ embryos
M.		44·620	44·510	45·061	44·723
P. E. M.	±	·077	·074	·056	·058
σ		1·1470	1·3189	1·0722	1·0926
P. E. σ	±	·0547	·0522	·0401	·0413
C. V.		2·57	2·96	2·38	2·44

TABLE 17.

Half Vertebrae	*32*	*33*	*34*	*35*	*36*	*37*	*38*	*39*	*40*	*41*	*42*	*43*	*44*	*45*	*46*	*47*	*48*	No.
♂ adults	—	—	—	—	1	6	11	12	25	18	13	9	3	2	—	—	—	*100*
♂ embryos	1	—	—	—	3	5	18	26	43	18	18	7	4	1	1	—	—	*145*
♀ adults	—	—	—	—	2	12	16	26	29	34	27	13	1	1	1	—	1	*163*
♀ embryos	—	—	—	1	3	6	24	22	46	30	17	6	4	—	—	—	—	*159*

		♂ adults	♂ embryos	♀ adults	♀ embryos
M.		40·370	40·034	40·288	39·956
P. E. M.	±	·128	·107	·101	·091
σ		1·9008	1·9169	1·9209	1·7022
P. E. σ	±	·0907	·0759	·0718	·0644
C. V.		4·71	4·79	4·77	4·26

TABLE 18.

Total Segments	*62*	*63*	*64*	*65*	*66*	*67*	*68*	No.
♂ adults	—	6	23	43	18	9	1	*100*
♂ embryos	1	21	40	49	25	7	2	*145*
♀ adults	—	2	24	65	44	23	5	*163*
♀ embryos	1	11	44	61	30	12	—	*159*

		♂ adults	♂ embryos	♀ adults	♀ embryos
M.		65·040	64·724	65·472	64·906
P. E. M.	±	·0707	·065	·055	·056
σ		1·0480	1·1536	1·0410	1·0449
P. E. σ	±	·0499	·0457	·0389	·0395
C. V.		1·612	1·782	1·590	1·610

TABLE 19.

Collector Nerves	*4*	*5*	*6*	*7*	No.
♂ adults	21	113	65	1	*200*
♀ adults	16	176	102	2	*296*

		♂ s	♀ s
M.		5·230	5·304
P. E. M.	±	·030	·024
σ		·6301	·6059
P. E. σ	±	·0212	·0168
C. V.		12·05	11·42

TABLE 20.

Post-Girdle Nerves	*6*	*7*	*8*	*9*	*10*	*11*	No.
♂ adults	—	1	18	102	66	13	*200*
♀ adults	7	71	163	49	6	—	*296*

		♂ s	♀ s
M.		9·360	7·920
P. E. M.	±	·039	·030
σ		·8188	·7580
P. E. σ	±	·0276	·0215
C. V.		8·748	9·570

TABLE 21.

1st G.-P. Nerve	*26*	*27*	*28*	*29*	*30*	*31*	No.
♂ adults	2	19	84	77	14	4	*200*
♀ adults	—	6	58	149	79	4	*296*

		♂ s	♀ s
M.		28·470	29·057
P. E. M.	±	·041	·030
σ		·8713	·7712
P. E. σ	±	·0294	·0213
C. V.		3·060	2·654

TABLE 22. *Correlations between whole Vertebrae and posterior Spine.*

TABLE 22. *Correlations between whole Vertebrae and posterior Spine.*

M.

	42	43	44	45	46	47	48	
39	—	5	3	2	—	—	—	10
40	1	9	20	10	1	—	—	41
41	—	1	8	20	8	—	—	37
42	—	—	1	—	6	2	—	9
43	—	—	—	—	—	1	2	3
	1	15	32	32	15	3	2	100

$r = \cdot 7317 \pm \cdot 0304$

F.

	43	44	45	46	47	48	
39	1	4	—	—	—	—	5
40	4	18	6	1	—	—	29
41	1	20	31	15	—	—	67
42	1	3	21	17	9	1	52
43	—	—	—	1	3	—	4
44	—	—	—	—	—	2	2
	7	45	58	34	12	3	159

$r = \cdot 6540 \pm \cdot 0297$

m.

	41	42	43	44	45	46	47	48	49	50	51	
39	1	4	8	5	—	—	—	—	—	—	—	18
40	—	3	10	34	15	—	—	—	—	—	—	62
41	—	—	1	8	25	11	—	—	—	—	—	45
42	—	—	—	1	2	8	7	—	—	—	—	18
43	—	—	—	—	—	1	—	—	—	—	1	2
	1	7	19	48	42	20	7	—	—	—	1	145

$r = \cdot 7742 \pm \cdot 0218$

f.

	42	43	44	45	46	47	48	
38	—	1	—	—	—	—	—	1
39	1	4	2	1	—	—	—	8
40	—	14	31	12	3	—	—	60
41	—	—	16	31	15	1	—	63
42	—	—	1	7	12	4	1	25
43	—	—	—	—	—	2	—	2
	1	19	50	51	30	7	1	159

$r = \cdot 7219 \pm \cdot 0248$

TABLE 23. *Correlations between total Segments and posterior Spine.*

M.

	63	64	65	66	67	68	
39	3	5	2	—	—	—	10
40	2	13	21	5	—	—	41
41	1	5	19	10	2	—	37
42	—	—	1	3	5	—	9
43	—	—	—	—	2	1	3
	6	23	43	18	9	1	100

$r = \cdot 6753 \pm \cdot 0356$

F.

	63	64	65	66	67	68	
39	—	4	1	—	—	—	5
40	2	10	14	3	—	—	29
41	—	7	41	16	3	—	67
42	—	2	8	24	16	2	52
43	—	—	—	1	2	1	4
44	—	—	—	—	—	2	2
	2	23	64	44	21	5	159

$r = \cdot 6754 \pm \cdot 0282$

m.

	62	63	64	65	66	67	68	
39	1	8	6	3	—	—	—	18
40	—	13	25	21	3	—	—	62
41	—	—	8	20	16	1	—	45
42	—	—	1	5	6	4	2	18
43	—	—	—	—	—	2	—	2
	1	21	40	49	25	7	2	145

$r = \cdot 6832 \pm \cdot 0290$

f.

	62	63	64	65	66	67	
38	—	1	—	—	—	—	1
39	1	1	4	2	—	—	8
40	—	6	28	24	3	—	60
41	—	3	12	27	16	4	63
42	—	—	—	8	9	8	25
43	—	—	—	—	2	—	2
	1	11	44	61	30	12	159

$r = \cdot 5882 \pm \cdot 0316$

TABLE 24. *Correlations between total Segments and whole Vertebrae.*

M.

	63	64	65	66	67	68	
42	—	1	—	—	—	—	1
43	3	4	7	1	—	—	15
44	2	10	15	4	1	—	32
45	1	8	16	7	—	—	32
46	—	—	5	5	5	—	15
47	—	—	—	1	2	—	3
48	—	—	—	—	1	1	2
	6	23	43	18	9	1	100

$r = \cdot 5534 \pm \cdot 0454$

F.

	63	64	65	66	67	68	
43	—	4	1	1	1	—	7
44	2	13	27	4	—	—	46
45	—	5	24	20	9	1	59
46	—	2	13	12	8	—	35
47	—	—	—	7	4	2	13
48	—	—	—	—	1	2	3
	2	24	65	44	23	5	163

$r = \cdot 5660 \pm \cdot 0358$

m.

	62	63	64	65	66	67	68	
41	—	—	1	—	—	—	—	1
42	—	4	3	—	—	—	—	7
43	1	8	6	4	—	—	—	19
44	—	8	25	12	2	1	—	48
45	—	1	5	24	11	1	—	42
46	—	—	—	7	10	2	1	20
47	—	—	—	2	2	2	1	7
48	—	—	—	—	—	—	—	—
49	—	—	—	—	—	—	—	—
50	—	—	—	—	—	—	—	—
51	—	—	—	—	—	1	—	1
	1	21	40	49	25	7	2	145

$r = \cdot 6707 \pm \cdot 0299$

f.

	62	63	64	65	66	67	
42	1	—	—	—	—	—	1
43	—	7	10	2	—	—	19
44	—	2	22	22	4	—	50
45	—	2	12	24	9	4	51
46	—	—	—	12	12	6	30
47	—	—	—	1	4	2	7
48	—	—	—	—	1	—	1
	1	11	44	61	30	12	159

$r = \cdot 6492 \pm \cdot 0300$

Note. M above a table denotes adult ♂ s, F=adult ♀ s, m= ♂ embryos, and f= ♀ embryos.

TABLE 25. *Correlations between half and whole Vertebrae.*

M.

	36	*37*	*38*	*39*	*40*	*41*	*42*	*43*	*44*	*45*	
42	—	—	—	—	—	—	—	1	—	—	1
43	—	—	—	—	3	2	2	6	1	1	15
44	—	1	1	5	5	10	5	2	2	1	32
45	1	1	7	3	13	4	3	—	—	—	32
46	—	3	2	3	2	2	3	—	—	—	15
47	—	—	1	—	2	—	—	—	—	—	3
48	—	1	—	1	—	—	—	—	—	—	2
	1	6	11	12	25	18	13	9	3	2	100

$r = -\cdot5226 \pm \cdot0476$

F.

	36	*37*	*38*	*39*	*40*	*41*	*42*	*43*	*44*	*45*	*46*	*47*	*48*	
43	—	—	—	—	—	3	1	1	—	1	—	—	1	7
44	—	—	2	5	8	11	16	3	1	—	—	—	—	46
45	—	2	3	10	14	14	6	9	—	—	1	—	—	59
46	2	8	5	8	4	5	3	—	—	—	—	—	—	35
47	—	1	6	2	2	1	1	—	—	—	—	—	—	13
48	—	1	—	1	1	—	—	—	—	—	—	—	—	3
	2	12	16	26	29	34	27	13	1	1	1	—	1	163

$r = -\cdot4860 \pm \cdot0392$

m.

	32...36	*37*	*38*	*39*	*40*	*41*	*42*	*43*	*44*	*45*	*46*	
41	—	—	—	—	—	—	—	—	—	—	1	1
42	—	—	—	—	—	2	2	—	3	—	—	7
43	—	—	—	1	1	7	2	4	4	—	—	19
44	—	—	—	8	6	19	7	5	2	—	1	48
45	—	1	3	2	11	13	7	4	1	—	—	42
46	—	—	2	5	7	3	—	2	—	1	—	20
47	—	2	—	2	1	1	—	1	—	—	—	7
48	—	—	—	—	—	—	—	—	—	—	—	—
49	—	—	—	—	—	—	—	—	—	—	—	—
50	—	—	—	—	—	—	—	—	—	—	—	—
51	1	—	—	—	—	—	—	—	—	—	—	1
	1 ... 3	5	18	26	43	18	18	7	4	1	1	145

$r = -\cdot5580 \pm \cdot0374$

f.

	35	*36*	*37*	*38*	*39*	*40*	*41*	*42*	*43*	*44*	
42	—	—	—	—	—	1	—	—	—	—	1
43	—	—	—	—	1	6	5	5	1	1	19
44	—	—	—	2	5	17	16	6	3	1	50
45	—	2	2	10	11	13	4	5	2	2	51
46	—	—	3	9	5	7	5	1	—	—	30
47	1	—	1	3	—	2	—	—	—	—	7
48	—	1	—	—	—	—	—	—	—	—	1
	1	3	6	24	22	46	30	17	6	4	159

$r = -\cdot5028 \pm \cdot0388$

TABLE 26. *Correlations between posterior and anterior Spines.*

M.

	39	*40*	*41*	*42*	*43*	
15	6	13	2	—	—	21
16	2	25	27	5	1	60
17	—	3	7	3	2	15
18	—	—	1	1	—	2
	8	41	37	9	3	98

$r = \cdot5216 \pm \cdot0485$

F.

	39	*40*	*41*	*42*	*43*	
14	1	—	1	—	—	2
15	2	10	7	—	—	19
16	1	16	38	26	—	81
17	—	3	18	16	3	40
18	—	—	—	2	1	3
	4	29	64	44	4	145

$r = \cdot4828 \pm \cdot0420$

m.

	39	*40*	*41*	*42*	*43*	
15	2	13	7	2	—	24
16	14	46	33	10	1	104
17	2	3	5	4	1	15
18	—	—	—	2	—	2
	18	62	45	18	2	145

$r = \cdot2102 \pm \cdot0524$

f.

	38	*39*	*40*	*41*	*42*	*43*	
15	—	3	13	5	1	—	22
16	1	5	40	42	18	1	107
17	—	—	7	15	6	1	29
18	—	—	—	1	—	—	1
	1	8	60	63	25	2	159

$r = \cdot2719 \pm \cdot0485$

TABLE 27. *Correlations between total Segments and half Vertebrae.*

M.

F.

m.

f.

TABLE 27. *Correlations between total Segments and half Vertebrae.*

M.

	63	64	65	66	67	68	
18	1	—	—	—	—	—	1
19	2	8	5	1	1	—	17
20	3	10	16	5	2	1	37
21	—	4	15	7	5	—	31
22	—	1	7	4	—	—	12
23	—	—	—	1	1	—	2
	6	23	43	18	9	1	100

$r = \cdot 3745 \pm \cdot 0563$

F.

	63	64	65	66	67	68	
18	—	2	—	—	—	—	2
19	2	5	13	7	1	—	28
20	—	13	24	12	4	2	55
21	—	4	27	20	8	2	61
22	—	—	1	4	9	—	14
23	—	—	—	1	—	1	2
24	—	—	—	—	1	—	1
	2	24	65	44	23	5	163

$r = \cdot 4397 \pm \cdot 0414$

m.

	62	63	64	65	66	67	68	
32	—	—	—	—	—	1	—	1
33	—	—	—	—	—	—	—	—
34	—	—	—	—	—	—	—	—
35	—	—	—	—	—	—	—	—
36	—	1	—	2	—	—	—	3
37	—	—	3	2	—	—	—	5
38	1	8	2	5	2	—	—	18
39	—	1	6	11	7	1	—	26
40	—	7	19	13	3	1	—	43
41	—	2	2	7	7	—	—	18
42	—	2	4	5	4	2	1	18
43	—	—	—	4	2	1	—	7
44	—	—	3	—	—	—	1	4
45	—	—	—	—	—	1	—	1
46	—	—	1	—	—	—	—	1
	1	21	40	49	25	7	2	145

$r = \cdot 1945 \pm \cdot 0523$

f.

	62	63	64	65	66	67	
35	—	—	—	1	—	—	1
36	—	2	—	—	1	—	3
37	—	—	2	3	1	—	6
38	—	2	10	9	3	—	24
39	—	1	5	11	5	—	22
40	1	6	17	13	7	2	46
41	—	—	5	16	4	5	30
42	—	—	5	6	5	1	17
43	—	—	—	1	3	2	6
44	—	—	—	1	1	2	4
	1	11	44	61	30	12	159

$r = \cdot 3266 \pm \cdot 0464$

TABLE 28. *Correlations between total Segments and anterior Spine.*

M.

	63	64	65	66	67	68	
15	2	7	9	3	—	—	21
16	4	10	27	11	8	—	60
17	—	5	5	3	1	1	15
18	—	—	1	1	—	—	2
	6	22	42	18	9	1	98

$r = \cdot 1544 \pm \cdot 0645$

F.

	63	64	65	66	67	68	
14	—	—	2	—	—	—	2
15	1	7	7	2	2	—	19
16	1	12	38	17	12	1	81
17	—	3	11	19	6	1	40
18	—	—	—	2	1	—	3
	2	22	58	40	21	2	145

$r = \cdot 328 \pm \cdot 0485$

m.

	62	63	64	65	66	67	68	
15	—	4	8	8	3	—	1	24
16	1	15	32	33	17	5	1	104
17	—	2	—	7	5	1	—	15
18	—	—	—	1	—	1	—	2
	1	21	40	49	25	7	2	145

$r = \cdot 1684 \pm \cdot 0528$

f.

	62	63	64	65	66	67	
15	—	4	6	10	1	1	22
16	1	4	32	46	18	6	107
17	—	3	6	5	10	5	29
18	—	—	—	—	1	—	1
	1	11	44	61	30	12	159

$r = \cdot 2246 \pm \cdot 0493$

TABLE 29. *Correlations between half Vertebrae and anterior Spine.*

M.

	18	19	20	21	22	23	
15	—	1	9	10	1	—	21
16	1	8	21	18	10	2	60
17	—	6	5	3	1	—	15
18	—	1	1	—	—	—	2
	1	16	36	31	12	2	98

$r = - \cdot 199 \pm \cdot 0635$

F.

	18	19	20	21	22	23	24	
14	—	—	1	1	—	—	—	2
15	—	4	6	5	3	1	—	19
16	2	15	24	32	8	—	—	81
17	—	5	15	17	2	—	1	40
18	—	1	—	2	—	—	—	3
	2	25	46	57	13	1	1	145

$r = \cdot 0031 \pm \cdot 0542$

m.

	16	17	18	19	20	21	22	23	
15	—	—	—	5	10	6	3	—	24
16	1	—	1	16	54	23	8	1	104
17	—	—	2	1	5	7	—	—	15
18	—	—	—	1	—	—	—	1	2
	1	—	3	23	69	36	11	2	145

$r = \cdot 0009 \pm \cdot 0543$

f.

	18	19	20	21	22	
15	—	1	12	9	—	22
16	3	24	42	29	9	107
17	1	5	13	9	1	29
18	—	—	1	—	—	1
	4	30	68	47	10	159

$r = - \cdot 068 \pm \cdot 0517$

TABLE 30.

Correlations between 1st Girdle-piercing Nerve and Post-girdle Nerves.

M.

	26	*27*	*28*	*29*	*30*	*31*	
7	—	—	—	—	1	—	1
8	—	—	1	10	3	4	18
9	—	—	38	54	10	—	102
10	—	10	43	13	—	—	66
11	2	9	2	—	—	—	13
	2	19	84	77	14	4	200

$r = -\cdot6227 \pm \cdot0283$

F.

	27	*28*	*29*	*30*	*31*	
6	—	—	—	5	2	7
7	—	—	35	35	1	71
8	2	26	97	37	1	163
9	2	28	17	2	—	49
10	2	4	—	—	—	6
	6	58	149	79	4	296

$r = -\cdot5758 \pm \cdot0254$

TABLE 31.

Correlations between 1st Girdle-piercing Nerve and posterior Spine.

M.

	26	*27*	*28*	*29*	*30*	*31*	
39	2	2	8	7	1	—	20
40	—	16	51	14	1	—	82
41	—	1	23	44	6	—	74
42	—	—	2	12	4	—	18
43	—	—	—	—	2	4	6
	2	19	84	77	14	4	200

$r = \cdot5768 \pm \cdot0309$

F.

	27	*28*	*29*	*30*	*31*	
39	—	7	3	—	—	10
40	2	26	24	—	—	52
41	4	17	69	28	—	118
42	—	8	48	40	—	96
43	—	—	—	6	2	8
44	—	—	—	2	2	4
	6	58	144	76	4	288

$r = \cdot5398 \pm \cdot0270$

TABLE 32.

Correlations between 1st Girdle-piercing Nerve and whole Vertebrae.

M.

	26	*27*	*28*	*29*	*30*	*31*	
42	—	2	—	—	—	—	2
43	2	6	16	5	1	—	30
44	—	8	41	15	—	—	64
45	—	1	22	36	5	—	64
46	—	2	5	19	4	—	30
47	—	—	—	2	3	1	6
48	—	—	—	—	1	3	4
	2	19	84	77	14	4	200

$r = \cdot6181 \pm \cdot0286$

F.

	27	*28*	*29*	*30*	*31*	
43	—	8	6	—	—	14
44	4	23	52	5	—	84
45	2	15	59	30	—	106
46	—	12	24	26	—	62
47	—	—	6	16	2	24
48	—	—	2	2	2	6
	6	58	149	79	4	296

$r = \cdot4548 = \pm \cdot0302$

TABLE 33.

Correlations between 1st Girdle-piercing Nerve and Collector Nerves.

M.

	26	*27*	*28*	*29*	*30*	*31*	
4	—	12	5	4	—	—	21
5	2	7	70	27	7	—	113
6	—	—	9	46	7	3	65
7	—	—	—	—	—	1	1
	2	19	84	77	14	4	200

$r = \cdot5590 \pm \cdot0318$

F.

	27	*28*	*29*	*30*	*31*	
4	—	12	4	—	—	16
5	6	36	101	33	—	176
6	—	10	44	44	4	102
7	—	—	—	2	—	2
	6	58	149	79	4	296

$r = \cdot3821 \pm \cdot0325$

TABLE 34.

Correlations between 1st Girdle-piercing Nerve and total Segments.

M.

	26	*27*	*28*	*29*	*30*	*31*	
63	2	4	4	2	—	—	12
64	—	6	24	16	—	—	46
65	—	9	41	32	4	—	86
66	—	—	12	18	6	—	36
67	—	—	3	9	4	2	18
68	—	—	—	—	—	2	2
	2	19	84	77	14	4	200

$r = \cdot5051 \pm \cdot0345$

F.

	27	*28*	*29*	*30*	*31*	
63	—	2	—	—	—	2
64	2	18	22	2	—	44
65	4	21	72	23	—	120
66	—	15	37	28	—	80
67	—	—	18	22	2	42
68	—	2	—	4	2	8
	6	58	149	79	4	296

$r = \cdot4253 \pm \cdot0312$

TABLE 35.

Correlations between total Segments and Collector Nerves.

M.

	63	*64*	*65*	*66*	*67*	*68*	
4	2	5	12	2	—	—	21
5	7	27	51	20	8	—	113
6	3	14	23	14	9	2	65
7	—	—	—	—	1	—	1
	12	46	86	36	18	2	200

$r = \cdot2056 \pm \cdot0443$

F.

	63	*64*	*65*	*66*	*67*	*68*	
4	—	3	6	5	—	2	16
5	2	28	74	50	22	—	176
6	—	13	39	25	19	6	102
7	—	—	1	—	1	—	2
	2	44	120	80	42	8	296

$r = \cdot102 \pm \cdot0376$

TABLE 36.

Correlation between number and proportional length of whole Vertebrae.

M.

	·605	*·615*	*·625*	*·635*	*·645*	*·655*	*·665*	*·675*	*·685*	*·695*	
43	1	1	2	4	1	—	—	—	—	—	9
44	—	1	4	2	5	7	1	—	—	—	20
45	—	—	1	2	2	2	4	—	—	1	12
46	—	—	—	1	3	3	2	—	2	—	11
47	—	—	—	—	—	—	1	—	—	—	1
48	—	—	—	—	—	—	1	1	—	—	2
	1	2	7	9	11	12	9	1	2	1	55

$r = \cdot5838 \pm \cdot0582$

TABLE 37.

Correlation between number of Fin Rays and Pelvic Nerves.

F.

	15	*16*	*17*	*18*	*19*	*20*	
12	—	2	9	4	—	—	15
13	5	13	13	15	3	2	51
14	1	5	8	4	3	—	21
15	—	—	2	1	2	—	5
	6	20	32	24	8	2	92

$r = \cdot058 \pm \cdot0680$

FRATERNAL CORRELATIONS.

TABLE 38.

Anterior Spine.

♂

	15	*16*	*17*	*18*	
15	12	34	5	—	51
16	34	258	47	4	343
17	5	47	24	—	76
18	—	4	—	—	4
	51	343	76	4	474

$r = \cdot 2543 \pm \cdot 0281$

♀

	15	*16*	*17*	*18*	
15	18	54	7	—	79
16	54	174	51	4	283
17	7	51	60	1	119
18	—	4	1	—	5
	79	283	119	5	486

$r = \cdot 3646 \pm \cdot 0257$

Siblings.

	15	*16*	*17*	*18*	
15	68	179	26	—	273
16	179	940	199	12	1330
17	26	199	130	7	362
18	—	12	7	—	19
	273	1330	362	19	1984

$r = \cdot 3085 \pm \cdot 0133$

TABLE 39.

Posterior Spine.

♂

	39	*40*	*41*	*42*	*43*	
39	34	18	2	—	—	54
40	18	80	76	26	2	202
41	2	76	46	19	4	147
42	—	26	19	16	2	63
43	—	2	4	2	—	8
	54	202	147	63	8	474

$r = \cdot 3863 \pm \cdot 0256$

♀

	39	*40*	*41*	*42*	
39	2	19	9	1	31
40	19	80	75	16	190
41	9	75	66	34	184
42	1	16	34	30	81
	31	190	184	81	486

$r = \cdot 3330 \pm \cdot 0264$

Siblings.

	39	*40*	*41*	*42*	*43*	
39	66	112	29	3	—	210
40	112	352	294	78	3	839
41	29	294	220	106	5	654
42	3	78	106	78	4	269
43	—	3	5	4	—	12
	210	839	654	269	12	1984

$r = \cdot 3732 \pm \cdot 0126$

TABLE 40.

Whole Vertebrae.

♂

	41	*42*	*43*	*44*	*45*	*46*	*47*	
41	—	5	—	—	—	—	—	5
42	5	20	—	—	—	—	—	25
43	—	—	4	17	9	6	—	36
44	—	—	17	44	38	21	3	123
45	—	—	9	38	78	38	9	172
46	—	—	6	21	38	18	8	91
47	—	—	—	3	9	8	2	22
	5	25	36	123	172	91	22	474

$r = \cdot 5407 \pm \cdot 0213$

♀

	43	*44*	*45*	*46*	*47*	
43	14	19	10	3	—	46
44	19	56	42	27	1	145
45	10	42	60	48	3	163
46	3	27	48	40	5	123
47	—	1	3	5	—	9
	46	145	163	123	9	486

$r = \cdot 3711 \pm \cdot 0256$

Siblings.

	41	*42*	*43*	*44*	*45*	*46*	*47*	
41	—	5	3	2	—	—	—	10
42	5	20	15	11	—	—	—	51
43	3	15	30	75	38	26	1	188
44	2	11	75	228	183	94	5	598
45	—	—	38	183	264	160	23	668
46	—	—	26	94	160	114	20	414
47	—	—	1	5	23	20	6	55
	10	51	188	598	668	414	55	1984

$r = \cdot 4049 \pm \cdot 0123$

These tables are to be read thus: There were 51 pairs of brothers with anterior spine at the 15th vertebra in the case of one of the pair, the other brother had the anterior spine at 15 in 12 cases, at 16 in 34 cases and at 17 in 5 cases; and so on.

TABLE 41.

Half Vertebrae.

♂

	36	*37*	*38*	*39*	*40*	*41*	*42*	*43*	*44*	*45*	*46*	
36	—	—	2	2	3	1	—	1	—	—	—	9
37	—	6	3	7	1	5	—	—	—	—	—	22
38	2	3	4	9	8	11	3	3	—	—	—	43
39	2	7	9	14	31	12	13	2	1	—	—	91
40	3	1	8	31	44	13	21	5	2	—	—	128
41	1	5	11	12	13	14	10	2	4	—	2	74
42	—	—	3	13	21	10	10	6	3	—	—	67
43	1	—	3	2	5	2	6	2	—	—	—	21
44	—	—	—	1	2	4	3	—	2	—	2	14
45	—	—	—	—	—	—	—	—	—	—	—	—
46	—	—	—	—	—	2	1	—	2	—	—	5
	9	22	43	91	128	74	67	21	14	—	5	474

$r = \cdot2621 \pm \cdot0352$

♀

	37	*38*	*39*	*40*	*41*	*42*	*43*	*44*	
37	2	6	9	5	1	—	—	—	23
38	6	6	16	13	11	3	1	1	57
39	9	16	20	24	10	5	3	—	87
40	5	13	24	24	37	21	7	4	135
41	1	11	10	37	24	14	7	1	105
42	—	3	5	21	14	2	3	2	50
43	—	1	3	7	7	3	—	—	21
44	—	1	—	4	1	2	—	—	8
	23	57	87	135	105	50	21	8	486

$r = \cdot2771 \pm \cdot0268$

Siblings.

	36	*37*	*38*	*39*	*40*	*41*	*42*	*43*	*44*	*45*	*46*	
36	—	1	4	6	4	1	1	1	—	—	—	18
37	1	8	14	19	21	9	—	1	1	—	—	74
38	4	14	26	50	52	43	14	6	1	—	—	210
39	6	19	50	52	112	55	35	15	6	—	1	351
40	4	21	52	112	159	103	81	27	15	—	2	576
41	1	9	43	55	103	59	52	20	6	—	2	350
42	1	—	14	35	81	52	36	18	11	—	3	251
43	1	1	6	15	27	20	18	8	1	—	—	97
44	—	1	1	6	15	6	11	1	4	—	2	47
45	—	—	—	—	—	—	—	—	—	—	—	—
46	—	—	—	1	2	2	3	—	2	—	—	10
	18	74	210	351	576	350	251	97	47	—	10	1984

$r = \cdot2313 \pm \cdot0139$

TABLE 42.

Total Segments.

♂

	63	*64*	*65*	*66*	*67*	*68*	
63	12	24	12	4	—	—	50
64	22	24	30	18	1	—	95
65	12	30	80	52	13	2	189
66	4	18	52	16	11	2	103
67	—	1	13	11	2	2	29
68	—	—	2	2	2	2	8
	50	95	189	103	29	8	474

$r = \cdot3950 \pm \cdot0254$

♀

	63	*64*	*65*	*66*	*67*	
63	2	13	6	3	—	24
64	13	32	44	22	2	113
65	6	44	98	41	11	200
66	3	22	41	20	14	100
67	—	2	11	14	22	49
	24	113	200	100	49	486

$r = \cdot4101 \pm \cdot0247$

Siblings.

	63	*64*	*65*	*66*	*67*	*68*	
63	36	73	41	15	1	—	166
64	73	144	164	73	10	—	464
65	41	164	364	175	43	2	789
66	15	73	175	80	46	6	395
67	1	10	43	46	48	6	154
68	—	—	2	6	6	2	16
	166	464	789	395	154	16	1984

$r = \cdot4285 \pm \cdot0120$

PARENTAL CORRELATIONS.

TABLE 43.

Anterior Spine.

Mother

Offspring.	*15*	*16*	*17*	
15	6	16	12	34
16	23	77	53	153
17	—	4	31	35
18	—	1	1	2
	29	98	97	224

$r = \cdot 359 \pm \cdot 039$

TABLE 44.

Whole Vertebrae.

Mother.

Offspring.	*44*	*45*	*46*	*47*	
41	—	—	1	—	1
42	—	—	5	—	5
43	7	11	7	—	25
44	17	24	17	—	58
45	20	26	26	3	75
46	8	11	22	3	44
47	1	—	2	3	6
	53	72	80	9	224

$r = \cdot 168 \pm \cdot 044$

TABLE 45.

Posterior Spine.

Mother.

Offspring.	*39*	*40*	*41*	*42*	
39	1	1	12	7	21
40	5	12	65	12	94
41	1	9	51	17	78
42	—	4	10	16	30
43	—	—	—	1	1
	7	26	138	53	224

$r = \cdot 196 \pm \cdot 043$

TABLE 46.

Half Vertebrae.

Mother.

Offspring.	*37*	*38*	*39*	*40*	*41*	*42*	*43*	
36	1	—	—	—	—	1	—	2
37	4	4	1	—	—	—	—	9
38	5	5	6	2	2	3	1	24
39	8	6	2	7	9	6	1	39
40	6	7	10	15	14	12	3	65
41	2	2	10	7	10	5	3	39
42	—	—	5	9	7	6	—	27
43	3	—	2	1	5	1	—	12
44	—	1	1	2	2	—	—	6
45	—	—	—	—	—	—	—	—
46	—	—	—	1	—	—	—	1
	29	25	37	42	49	34	8	224

$r = \cdot 238 \pm \cdot 042$

TABLE 47.

Total Segments.

Mother.

Offspring.	*63*	*64*	*65*	*66*	*67*	*68*	
63	—	6	7	6	—	—	19
64	—	11	25	17	1	—	54
65	6	8	42	29	5	1	91
66	—	7	14	13	6	3	43
67	—	1	—	3	8	3	15
68	—	—	—	—	—	2	2
	6	33	88	68	20	9	224

$r = \cdot 416 \pm \cdot 037$

Note on Mr Punnett's Section on the Inheritance of Meristic Characters.

BY KARL PEARSON.

I should like to add a few words (as he himself has suggested) as to the results of this portion of Mr Punnett's investigation. I feel the more anxious to do so because he has broken entirely new ground, which we can only hope will attract the attention it deserves, and so bring new workers to this field. If so, it is desirable to state the conditions under which it is possible to obtain final results. The father in the case chosen by Mr Punnett is quite unknown, hence it is absolutely needful, in order to reach accurate values of the correlation, that we should have enough mothers of each class to insure that the average father of that class is practically the average father of the population. When Mr Punnett first sent me material for reduction it consisted of 88 individuals forming 10 families for maternal correlation, and 93 individuals in 12 families for fraternal correlation. Allowing for four to five classes in the tables, it was obvious that the extreme arrays of offspring would be due to one, or at most two mothers. It was not likely, therefore, that the mean father of these groups would be the mean father of the general population. However, we ventured to reduce the data with the following results :—

Correlations.

Series I.	88 Individuals in 10 Families		93 Individuals in 12 Families		
Character	Parental Correlations		Fraternal Correlations		
	Raw Value	Corrected Value (a)	Raw Value	Corrected (a)	Corrected (b)
Posterior spine ...	·440 ± ·058	·534	·150 ± ·023	·246	·342
Anterior spine ...	·404 ± ·060	·444	·488 ± ·018	·509	·530
Whole vertebrae ...	·369 ± ·062	·467	·297 ± ·022	·364	·431
Half vertebrae ...	·225 ± ·068	·281	·152 ± ·023	·177	·203
Total segments ...	·534 ± ·051	·706	·294 ± ·022	·504	·714
Mean ...	·396	·486	·276	·360	·444

(a) Corrected for selection of mothers out of adult ♀ population.

(b) Corrected for selection of fathers also, on assumption that they were equally selected with mothers, although unknown.

Corrections made in the manner indicated *Phil. Trans.* Vol. 200, A, pp. 39—45.

Now it will be seen at once that the average corrected parental correlation, i.e. ·49, is in essential agreement—not with the ancestral law which fixes no value for the correlation—but with the values we have found for man, horse, and dog. On the other hand the fraternal correlation is very low, ·44 or even ·36, a value lower than we have yet found for any *physical* character in

a *long* series of mammals. I told Mr Punnett that this value alone seemed sufficient to show that the material was inadequate. He then added 136 individuals in 15 families. They gave :

Series II.	136 Individuals in 15 Families		136 Individuals in 15 Families		
Character	Parental Correlations		Fraternal Correlations		
	Raw Value	Corrected Value (a)	Raw Value	Corrected (a)	Corrected (b)
Posterior spine ...	·096 ± ·057	·142	·473 ± ·015	·479	·484
Anterior spine ...	·357 ± ·050	·396	·269 ± ·018	·294	·318
Whole vertebrae ...	·139 ± ·057	·177	·469 ± ·015	·475	·482
Half vertebrae ...	·232 ± ·055	·252	·238 ± ·019	·246	·253
Total segments ...	·379 ± ·050	·332	·477 ± ·015	·457	·437
Mean ...	·241	·260	·385	·390	·395

The results were now much lower for the parental correlations, and slightly lower for the fraternal correlations. The differences between the two sets are in a number of cases significant compared with the probable error of the differences, and the only safe conclusion to be drawn is that the two samples from two different years are not *random* samples of the same population. Mr Punnett tells me that he cannot fix on any difference in the method of collecting or observing the two series. Judging by p. 321 one appears to have been procured at a rather earlier date than the other. Clubbing the two sets together we had the values for fraternal and parental correlation given in Mr Punnett's Tables 8 and 9. These arise from 224 individuals in 25 families for the parental, and 229 individuals in 27 families for fraternal values. The general result is, as we might expect, midway between the two.

Can we venture to draw any definite conclusions from these divergent values, and, if so, what?

Now, I think we may safely say that Mr Punnett has demonstrated the existence of inheritance in these meristic characters ; but that the paucity of material does not allow us to definitely fix its intensity.

The values found are very similar in intensity to those first found for stature in man from Mr Galton's material, mothers and sons ·302, mothers and daughters ·284*. These were based on 200 families. Again from the same data : Brother and brother = ·391, sister and sister ·444, sister and brother ·375. On Mr Galton's hypothesis the ancestral law gives for parental correlation ·300, and for fraternal ·400†. Or, we might even say that Mr Punnett's fish results agree on the average fairly well with those of Mr Galton for stature, and confirm his view of the ancestral law‡.

But this is, I take it, to miss the real point at issue. Such low values as Mr Punnett finds have been found before in man, in lepidoptera, and in molluscs, but in all these cases in short and doubtful series. They tell neither for nor against the ancestral law, but they are opposed to any hypothesis as to sensibly equal parental correlation in different species and for a variety of characters. That is a very vital point which no one can at present consider finally settled.

* *Phil. Trans.* A, Vol. 187, p. 270 and p. 281.

† *R. S. Proc.* Vol. 62, pp. 397 and 410.

‡ Similarly we have for a short series of 100 families, the inheritance of cephalic index, mothers and daughters ·300 and brothers and sisters ·340, for example. *R. S. Proc.* Vol. 62, p. 415.

Still in long series for a variety of characters in man, horse, and dog the parental correlation certainly approaches a value about ·5. If Mr Punnett's values were for a long series I think we should have to conclude that the intensity of heredity could vary very much from species to species, and perhaps admit even considerable variation in the same species for the same character at different periods. Hitherto, however, we have found that when we replace our short and doubtful series by longer and more carefully measured or observed data—as we have done in several of the cases referred to above—we approach closer to the value ·5 for parental heredity. Further, the confirmation of Mr Punnett's statements on p. 333, as to growth changes in post-embryonic life (and perhaps during embryonic life itself?) would certainly lead us to expect a weakened parental correlation (and in the latter case a weakened fraternal correlation). Anyhow values like ·3 for parental and ·4 for fraternal correlation cannot discriminate between the ancestral law and any form of Mendelian theory, for the former with Mr Galton's value of the constants actually leads to these values. A value like ·5 would be opposed to *one* fairly comprehensive Mendelian theory, but possibly not to every such theory.

A possible test between the ancestral law and my generalized Mendelian theory is given in my paper: *On a Criterion which may serve to test various Theories of Inheritance**. It is there indicated that we should expect the variability of the several arrays in the latter case to be given by a parabola, while with anything like a normal distribution of variations it would in the former case be a straight line. Dr Lee and I applied this test to Mr Punnett's five parental correlation tables, we calculated the S.D.'s of each array and plotted them to the magnitude of the parental character. But the numbers in the arrays were so small, and therefore their probable errors so large, and there were so few points (especially if we omitted those which belonged to arrays of 6 to 10 members only, and therefore were absolutely untrustworthy), that we got nothing but wild zigzags of three to four lines; these differed between the wide limits of random sampling from neither straight line nor parabola, and were quite insufficient to serve as any criterion. In fact they only again emphasized the lesson we had already learnt from short series that 25, 50, or even 100 families are insufficient to adequately determine the biometric constants of a correlation surface for heredity. In saying this I do not for a moment undervalue the importance of Mr Punnett's work; he has shown for the first time how heredity in one species of fish can be dealt with, and this is a great step onwards. I quite realize the difficulty of dealing with 1000 mothers, but this is, I believe, the sort of ideal we must biometrically propose if we are to reach data sufficiently smooth to give final values to the heredity coefficients, and so available for testing between current theories.

* *R. S. Proc.* Vol. 73, pp. 262—80.

ON THE MEASUREMENT OF INTERNAL CAPACITY FROM CRANIAL CIRCUMFERENCES.

By M. A. LEWENZ, M.A., and KARL PEARSON, F.R.S.

(1) In a memoir by Dr Alice Lee* on the reconstruction of the capacity of the skull from external measurements the following results were obtained:

(*a*) Equations giving the probable capacity of the skull from a knowledge of the length, breadth and height for a number of races.

(*b*) Equations giving the probable capacity from a knowledge of the product of length, breadth and height.

(*c*) A demonstration of the small relationship between capacity and cephalic index.

(*d*) A demonstration that the relationship between capacity and both horizontal and vertical circumferences varied much from race to race, even between allied races.

The conclusions drawn from these results were:

(i) That on the data dealt with there was no means of getting a small probable error in predicting the capacity of an individual skull from external measurements, but good results might be found from the average of series.

(ii) That (*b*) gave a formula which changed less from race to race than (*a*), and the mean formula from (*b*) gave reasonably close results when we paid attention to sex difference.

(iii) That the formula for the nearest allied race should, where possible, be selected, and failing any knowledge of evolutionary history then the mean formula based on (*b*).

It will be clear that the above results emphasise the need for judicious caution in using the cranial capacity formulae, and also offer hints from experience to those anxious to discover new formulae.

* *Phil. Trans.* Vol. 196, A, pp. 225—264.

(2) Dr J. Beddoe has recently taken up the relation of skull capacity to cranial circumferences and published* new formulae connecting the capacity with the product of three such circumferences. Dr Beddoe is a veteran who has done such good service in creating interest in anthropometric matters, that we should hardly have noticed his paper hostilely here, had he not (*a*) shown in it a complete ignorance of the nature of modern statistical theory, and (*b*) misinterpreted partly through misunderstanding and partly through faulty arithmetic Dr Lee's results.

There is absolutely no reason why the product of three cranial circumferences should not be taken as a basis for estimating the capacity of the skull, but there is only one scientific way of reaching a suitable formula and that Dr Beddoe has not adopted.

We will point out what considerations must guide us in the matter. Let us take first the following three circumferences of the *Frankfurter Verständigung*, U = horizontal circumference, S = sagittal circumference from nasion to opisthion, Q = transverse circumference from the top of one auricular passage to the top of the other in the vertical plane, i.e. the plane which is perpendicular to the "horizontal plane," and does not generally pass through the bregma. Let C be the capacity, and $P = U \times S \times Q$, then we require to find a relation between C and P. In order to do this we must measure C and P for as many skulls of one sex and race as possible and then for as many races as possible to see how our formula changes with race.

The process may be illustrated as follows. We take 164 Theban mummy skulls and pick out all those with C lying between 1380 and 1390 cubic centimetres. This is a group well within the probable error of capacity determination. We have 10 such skulls with the values for P in cubic centimetres given below. Now 10 cubic centimetres is about 1/11 of the standard deviation in capacity and we may take 500 cubic centimetres to be very roughly the same proportion of the variability of P. Picking out all the P's from 58,600 to 59,100 we have the following system:

Capacities from 1380—1390		P from 58,600 to 59,100	
Values of P in cms.^{3}	53,407	Values of C in cms.^{3}	1310
	54,432		1353
	54,568		1355
	57,652		1360
	58,892		1365
	59,369		1380
	60,194		1426
	60,277		1430
	60,596		1437
	62,390		1470
Mean	58,208	Mean	1389

* "De l'Évaluation et de la Signification de la Capacité cranienne," *L'Anthropologie*, Vol. XIV. pp. 267—294, 1903.

Now it will be seen at once from these results that to say a skull has a certain capacity does not fix the product of the circumferences, and to say that it has a certain product of circumferences by no means fixes its capacity. The only thing that is possible for us to assert is that Theban skulls with a capacity of 1380—90 will cluster round the value 58,208 of the circumference product, and skulls with a circumference product between 58,600 and 59,100 will cluster round the capacity value 1389. If these clusters follow—and they do follow—the usual distribution of characters in man, then these mean values are the "most probable values" for product or capacity when deduced from a knowledge of capacity or product respectively. Tables can now be dressed giving these "most probable values" for each small range of capacity or product. Such tables are only another form of the correlation table and the regression curve, which Dr Beddoe seems to find so mysterious; they are the only scientific way in 1904 of approaching these questions. Such "most probable values" in the case of most characters in man are found—allowing for the errors of random sampling—to increase uniformly with the uniform increase of the known character, in other words the regression curve is usually a straight line. The equation to this straight line is found by perfectly easy mathematical work and is what Dr Lee gave in her paper for the product of the three diameters, and what we give in this paper for the product of the three circumferences. If such a *linear* formula is to be used at all this is the only scientific way of approaching the problem. Dr Beddoe himself uses such a linear relation, B_1; yet how does he write of the only method of scientifically dealing with a perfectly elementary statistical problem?

"Je me reconnais incompétent pour décider si l'idée de corrélation peut tromper quand il s'agit de la forme cranienne et de sa capacité, et si l'idée de compensation, moins distinctement appréciable, il est vrai, mérite d'être étudiée*."

We assert, with a distinct appreciation of the seriousness of the statement, that a writer who cannot realise what correlation is and how it is used in a simple problem of this kind has no right nowadays to deal with craniometric problems at all. It is only by frankly asserting on every occasion this truth that we can hope to render anthropometry in all its branches a real science. The day for the old methods is once and for ever gone. Correlation is simply the mathematical process of finding the best linear relation between the known value of one character and the most probable value of a second. It determines in the special case A and B in the relation:

$$\text{Probable } C = A + B \times \text{Known } P \quad \text{.....(i).}$$

Dr Beddoe also wants to find a linear relation between Probable C and a Known P. How does he proceed? In no case does he go through the laborious and necessary work of tabling the mean C for a given small range of P. He simply puts:

$$\text{Probable } C = B \times \text{Known } P \quad \text{.....(ii),}$$

* *Loc. cit.* p. 278.

and *guesses* what he considers a suitable value for B! He therefore (1) assumes that his line holds far beyond the range of any actual observation*, for his only ground for putting $A=0$ must be, although he does not state it, that C vanishes with P; (2) neglects the result of all observations hitherto made on human characters, for in no case, that we are aware of—and certainly more than 100 must now have been worked out—is $A=0$. When Dr Beddoe does not guess B he obtains its value by giving C and P their mean values. Even here as he does not know the mean value of C, he guesses it!

"Je suis bien certain que la moyenne des pesées de cerveaux d'Anglais est un peu au dessous de 1400 grammes et que la moyenne de la capacité de crânes masculins anglais est voisine de 1500 c.c.†"

The only reasonably long series of English skulls with which we are acquainted neither give a mean of 1500. This is the type of guesswork which has hitherto passed for science in anthropometry. Even if Dr Beddoe had found by actually examining data that a formula

$$\text{Probable } C = B \times \text{Known } P$$

held for the skulls of one race, he ought before adopting it generally to have noted Dr Lee's warning about the changing degree of correlation between circumferences and capacity when we pass from one race to a closely allied race, and accordingly have tested it on other races. He has not attempted this, but finding it did not agree very well with observations on individuals attempted to modify it by introducing corrections depending on the cephalic index. This he has done notwithstanding Dr Lee's result that there is generally little correlation between cephalic index and capacity, and that what there is *varies largely from race to race*‡.

(3) We now turn to our own contribution to the subject. Dealing with the three circumferences U, S, Q defined above, which differ somewhat from Dr Beddoe's three, we have sought a linear relationship between their product P and the mean value of the corresponding C for four different races. We have used the following male crania:

(*a*) Fawcett's measurements on 98 Naqada crania.

(*b*) E. Schmidt's measurements on 164 Theban mummies.

(*c*) Ranke's measurements on 78 Altbaierisch crania.

(*d*) Macdonell's measurements§ on 72 English crania,

and we have one series for female crania,

(*e*) Macdonell's measurements on 76 English crania.

* While the relation of the characters in man is closely linear for the range found in normal individuals of one race, it certainly ceases to be linear when we pass to 'dwarfs' and 'giants.' See Pearson: *Phil. Trans.* Vol. 192, A, pp. 169—214. There is no reason whatever for extending the linear relation like (i) to extreme 'dwarfs' and putting $A=0$.

† *Loc. cit.* 285.

‡ See *loc. cit.* p. 233.

§ We have heartily to thank Dr Macdonell for providing us not only with the data, but the actual reductions in cases (*d*) and (*e*). His own memoir on the English skull appeared in *Biometrika*, Vol. III. pp. 191—244.

The following results were obtained:

TABLE I.

Race	Mean C	Mean P	σ_C	σ_P	r_{CP}
(*a*) Naqada ♂ ...	1375	57,414	108·52	4412	·7492
(*b*) Theban ♂ ...	1388	58,285	118·01	4863	·8576
(*c*) German ♂ ...	1506	64,604	111·54	4997	·8587
(*d*) English ♂ ...	1477	60,860	122·37	5206	·8790
(*e*) English ♀ ...	1300	53,656	112·80	5157	·8860

From these results the following equations flow:

(*a*) Naqada ♂ Probable $C = 317{\cdot}2 + {\cdot}01843P$
(*b*) Theban ♂ Probable $C = 174{\cdot}7 + {\cdot}02081P$
(*c*) German ♂ Probable $C = 267{\cdot}4 + {\cdot}01917P$
(*d*) English ♂ Probable $C = 219{\cdot}2 + {\cdot}02067P$
(*e*) English ♀ Probable $C = 263{\cdot}9 + {\cdot}01913P$

C and P being measured in cm.³

Now from these equations certain results at once flow. It is clear:

(i) That it is absolutely illegitimate to assume like Dr Beddoe that $A = 0$.

(ii) That A varies widely with race and sex, while B varies much less markedly, but possibly significantly. This is precisely what we might expect had racial differentiation arisen by selection from a common stock.

Thus Dr Lee's statement that the capacity and circumferences correlations varied so much between closely allied races that we could hardly expect to deduce a formula true for all races is fully borne out, when we investigate the relation between capacity and the circumference product. Dr Lee, after showing that the capacity and horizontal and vertical circumference correlations and regression equations varied from race to race, remarked: "We conclude therefore that it appears unlikely that a reconstruction formula, based on the circumferential measurements of the skull, can be found which will give good results, if extended from one local race to another*."

Dr Beddoe has apparently quite misunderstood the nature of the argument and simply dismisses the difficulty without meeting it with the words: "Le Dr Lee a fait des expériences sur l'arc transverse, mais elle a négligé l'arc sagittal, et ses résultats ne sont guère satisfaisants†."

Dr Lee's series seem sufficient for her purpose; it is the fact that they are sufficient but not satisfactory, which Dr Beddoe has no right to shirk.

If the reader will compare the above male formulae with those obtained for three races between the capacity and the diameters' product by Dr Lee‡, he will

* See *loc. cit.* p. 263.

† *Loc. cit.* p. 269. Dr Lee used *two* arcs, not merely one as is suggested.

‡ See *loc. cit.* p. 243.

at once realise why Dr Lee considered a diametral product formula could be better extended from one local race to another than a multiple regression circumference formula. She did not attempt a product circumference formula.

In view of the fact that our formulae vary so with race, it is venturesome to form a mean equation and treat it as an "inter-racial formula," but for comparison with Dr Beddoe's results it is perhaps desirable to do so*. We have as mean formula for males:

$$\text{Probable } C = 244{\cdot}6 + {\cdot}01977\,P \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (f).$$

We would only warn our readers to use such a formula with caution. For example, never to use it for females, and only in preference to one of the above four formulae when no evolutionary race relationship can be predicted between the above races and the race to be investigated.

We now turn to the probable error of these results. We have:

Probable Error of Individual Prediction.

Formula	(a)	(b)	(c)	(d)	(e)	Mean
Probable Error ...	48	41	39	39	35	40·4

To test the value of these results, 15 ♂ skulls (as nearly as possible every thirtieth skull) were taken at random from the Naqada measurements; 15 ♂ skulls from the Theban records (as nearly as possible each tenth skull), and similarly 15 from the Altbaierisch series. In the case of the Naqada crania 8 had an error greater and 7 less than the probable error; in the case of the Theban the numbers were 6 greater and 9 less, and for the Altbaierisch 9 greater and 6 less; or total 23 greater and 22 less than the probable error, which may be considered very satisfactory. The mean error of the 45 skulls as found from the mean formula is 47·5, corresponding to a probable error of $47{\cdot}5 \times {\cdot}8454 = 40{\cdot}2$. The theoretical value of this probable error is 40·4. There can therefore be, we think, small doubt that our mean formula will predict the capacity of a single *individual* skull from circumferential measurements with a probable error of about 40 cubic centimetres. Dr Lee's diametral product formula† showed a probable error of 43 for Aino ♂s, 55 for German ♂s and 52 for Naqada ♂s, say a mean value of 50. Actual application to 80 crania gave only a mean error of 49, or a probable error of about 42. Thus we should expect the present formulae to give slightly better results than the diameter product formulae, but for reasons to be given later we hold them quite inferior to those formulae for the living head.

* A more correctly *inter-racial* result is given on p. 386 below. The above is really a mean *intra-racial* result.

† See *loc. cit.* pp. 234–7.

(4) We now turn to Dr Beddoe's rule B_3, which up to the date of writing this he considers his best formula *.

If we put Dr Beddoe's rule into a formula it is, if C be the capacity in cubic centimetres:

$$C = \left(1 + \frac{1}{300}(I - 80)\right) \frac{U \times S' \times Q'}{36000},$$

where I is the cephalic index, U is the horizontal circumference, Q' is the auricular circumference from the *centre* of one auricular passage to that of the other, and S' the sagittal arc from nasion to inion, all in millimetres.

It may be written in the form:

$$C = \cdot 0000926\, I \times U \times S' \times Q' + \cdot 020374\, U \times S' \times Q',$$

where the product $U \times S' \times Q'$ is to be read in cubic centimetres and not cubic millimetres.

Now let p_1 stand for $U \times S' \times Q'$ and p_2 for $I \times U \times S' \times Q'$, and let σ_v be the standard deviation of the character or quantity v and r_{vw} the correlation of

* Dr Beddoe gives three formula in his paper B_1, B_2, B_3, and calculates some of the results by one and some by another of these rules. He tells us that he has another rule B_4 in preparation, and that he has been gradually feeling his way. It is against any such process as this that we wish emphatically to protest. Every one of Dr Beddoe's formulae is a mere guess. In no case does he adopt a certain type of formula and determine by recognised statistical methods the best values to be given to the constants of that formula. If he finds it gives him somewhat too high a series of values he merely reduces it by cutting off a series of percentages from the result. As he tells us B_3 is his best formula at present we have used B_3. This is designed to give what Dr Beddoe terms Flower's values, or what we should simply term the capacities properly measured, e.g. not the recognised erroneous values of Barnard Davis. It is obtained in the following manner: he measured his sagittal arc, S', from the nasion to the inion instead of to the opisthion—of this later more—and since the sagittal arc, S', is usually measured to the opisthion, he subtracts 50 mm. from it to find S'. Next he takes the vertical circumference, Q', which he measures from the *centre* of one auricular passage to that of the other. This is really not our Q. Our transverse circumference is measured from *top* of one auricular passage to the top of the other and "vertical," i.e. perpendicular to the Frankfurt "horizontal plane." He ought not accordingly to have interchanged as he has done our Q with his own Q'. Probably 9 or 10 mm. ought to be subtracted to get Q from Q', even if we could possibly accept his "passant par le bregma ou pas loin en arrière" as giving even an approximation to the "vertical plane." But as Dr Beddoe appears content to use our Q for his Q', we have made no attempt to modify it in order to reach an arc so vaguely described as "passing through the bregma or not far behind it." As a matter of fact the "vertical plane" may easily fall 10, 20, or even 30 mm. behind it. Lastly, to get the capacity Dr Beddoe multiplies $\frac{1}{3}$ of the horizontal circumference (U) by $\frac{1}{3}$ of the nasio-iniac arc S' by $\frac{1}{2}$ of his auricular arc. He then divides this product by $\frac{1}{3600}$, and deducts $\frac{1}{3}$ per cent. of the result for each unit of the cephalic index below 80, and adds a third per cent. for each unit above 80. This is the rule which we have represented by the formula in the text. It will be observed that it is by no means so simple as the product formulae we have used, besides being obtained by a theoretically unjustifiable process. B_1 takes 45 mm., not 50 mm., off S to get S' and neglects the cephalic index correction. B_2 takes 50 mm. off like B_3, but takes $\frac{1}{4}$ instead of $\frac{1}{3}$ per cent. for cephalic index correction. It will be seen that the constants are pure guesswork changeable at will.

the two quantities or characters v and w; then the proper formula for Dr Beddoe to have taken would have been:

$$C - m_c = \frac{r_{cp_1} - r_{cp_2} r_{p_1p_2}}{\sqrt{1 - r^2_{p_1p_2}}} \frac{\sigma_c}{\sigma_{p_1}} (p_1 - m_{p_1}) + \frac{r_{cp_2} - r_{cp_1} r_{p_1p_2}}{\sqrt{1 - r^2_{p_1p_2}}} \frac{\sigma_c}{\sigma_{p_2}} (p_2 - m_{p_2}) \ldots\ldots\ldots (g).$$

Dr Beddoe has assumed that:

$$m_c = \frac{r_{cp_1} - r_{cp_2} r_{p_1p_2}}{\sqrt{1 - r^2_{p_1p_2}}} \frac{\sigma_c}{\sigma_{p_1}} m_{p_1} + \frac{r_{cp_2} - r_{cp_1} r_{p_1p_2}}{\sqrt{1 - r^2_{p_1p_2}}} \frac{\sigma_c}{\sigma_{p_2}} m_{p_2}.$$

Such a result is contrary to all anthropometric experience*.

There is of course no reason why a formula like (g) should not be tried for skull capacity, but there is only one scientific method of determining it, namely the discovery of the actual means, standard deviations and correlations of the quantities involved for a long series of skulls. We cannot, in the light of modern statistical science, assume:

$$C = A + D_1 p_1 + D_2 p_2,$$

tacitly put $A = 0$, and take a series of guesses at D_1 and D_2, "feeling our way."

But the labour of investigating a formula like (g) is considerable, and no wise person would undertake it without first considering its chances of giving a remunerative result. Let us first enquire into this.

Formula (g) may be written if subscript i stands for I:

$$C = \gamma_0 + (\gamma_1 + \gamma_2 I)\, p_1,$$

whence:

$$\delta C = (\gamma_1 + \gamma_2 m_i)\, \delta p_1 + \gamma_2 m_{p_1} \delta I,$$

and ultimately:

$$\sigma_c r_{ci} = (\gamma_1 + \gamma_2 m_i)\, \sigma_{p_1} r_{p_1 i} + \gamma_2 m_{p_1} i\, \sigma_i,$$

$$\sigma_c = (\gamma_1 + \gamma_2 m_i)\, \sigma_{p_1} r_{cp_1} + \gamma_2 m_{p_1 i}\, \sigma_i r_{ci}.$$

Eliminating $\gamma_1 + \gamma_2 m_i$ we have:

$$\gamma_2 = -\frac{1}{m_{p_1}} \frac{\sigma_c}{\sigma_i} \frac{r_{p_1 i} - r_{ci}\, r_{cp_1}}{r_{cp_1} - r_{ci}\, r_{p_1 i}}.$$

Now we know that r_{ci} is usually very small, and therefore the terms $r_{ci}\, r_{cp_1}$ and $r_{ci}\, r_{p_1 i}$ will be small; further, r_{cp_1} is always positive. Hence γ_2 will be of *opposite* sign to $r_{p_1 i}$. Now, so far as we know, nobody has worked out the correlation of p_1 and I, but since the correlation of p_1 and C is positive and large there can be no reasonable doubt that that of p_1 and I will have the same sign as that of C and I. But the correlation of capacity and cephalic index is *negative* for dolichocephalic races and *positive* for brachycephalic races. This conclusion

* See, for example, the multiple regression formulae given for the long bones by K. Pearson, *Phil. Trans.* Vol. 192, A, pp. 186–7, for cranial measurements by A. Lee, *Phil. Trans.* Vol. 196, A, pp. 235–6, and for the like measurements by S. Jacobs, A. Lee, and K. Pearson, *Biometrika*, Vol. II. pp. 349—355. The list might be indefinitely extended.

had already been published in a memoir which has been read by Dr Beddoe. We have in fact the following results:

TABLE II.

Correlation of Capacity and Cephalic Index.

Race	Cephalic Index	Number	Correlation	
♂ Thebans ...	74·8	187	−·15	
♂ Naqadas ...	73·2	100	−·13	
♂ English ...	74·3	77	−·02*	−·15
♂ Aino	76·5	76	−·31	
♂ Copts	77·3	56	−·14	
♂ Etruscans ...	78·5	78	+·22	
♂ French ...	79·8	56	+·14	
♂ Malays ...	81·9	76	+·03	+·15
♂ Germans ...	83·3	100	+·20	

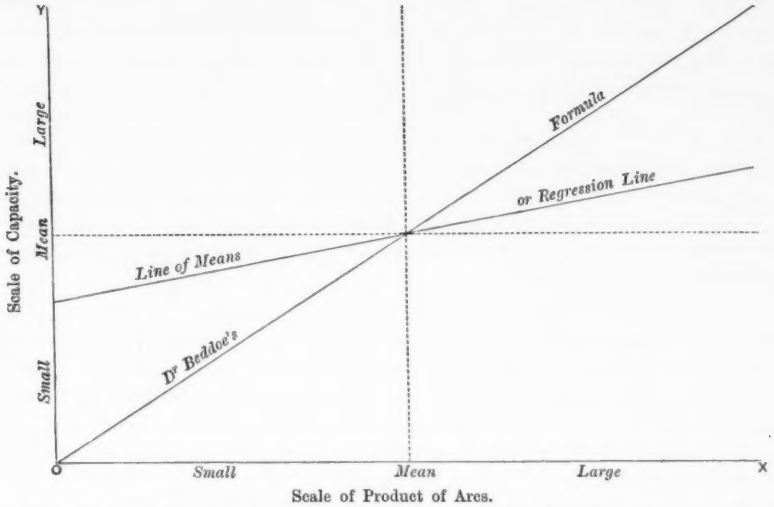
We see from this table (i) that the relationship between cephalic index and capacity is slight and irregular, and (ii) that any formula like Dr Beddoe's which not only puts γ_0 zero but gives a value to γ_2 persistently constant in magnitude and sign must be erroneous.

(5) Having shewn that Dr Beddoe's correction for cephalic index must be wrong for *individual* crania, when we pass from dolichocephalic to brachycephalic races, because in the former on the average the more dolichocephalic, in the latter the more brachycephalic individuals have the greater capacity, we turn to the main part of his expression (as it appears purely and simply in his first result B_1, in which he takes the capacity solely proportional to the product p_1) and ask whether this is reasonable. A little consideration will show how impossible it is that a formula of the type $C = D_1 p_1$ can at all agree with a formula of the type $C = A + D_1 p_1$ to which the true theory of correlation leads us. In the accompanying figure the two lines are indicated. It will be seen at once that except near the actual mean values of p and C Dr Beddoe's formula cannot possibly give the mean or most probable value of any array. If, however, a small skull be taken with thick walls, or a large skull with thin walls, then Dr Beddoe's formula naturally gives a better result in *extreme* cases. This is the source of any advantage which may appear in its favour when he proceeds to select large crania, "décrits comme minces ou transparents," or small skulls, "décrits comme épais ou lourds," and points out that for certain such cases his rule gives a better result than Dr Lee's correlation formula. This he has actually done in Tables II and III of his paper†. The answer to such criticism is that he has obtained his advantage by causing his line to give bad results for the capacity of the average skull of a given product, i.e. he has swung it round till it makes an angle with the line of "probable values." Thus his guess-work formula is really only a line diverging very considerably from

* Deduced from data provided by Dr Macdonell. The result shows that there is sensibly no correlation between cephalic index and capacity in the skulls here dealt with.

† *Loc. cit.* pp. 275 and 276.

the line of most probable values. If he must take a line at all then the only thing open to him is to use the scientific process of correlation and find the best fitting line.



Scale of Product of Arcs.

(6) We now pass to the arcs which Dr Beddoe has chosen for the basis of his formula. We take first his sagittal arc from nasion to inion. Dr Beddoe makes no reference whatever to the difficulty of determining the inion even on the skull. The inion is defined in the *Frankfurter Verständigung* as the point where the *linea superior nuchae* meets the median plane. But since the date of that concordat the further investigation of the matter has shown that this definition is extremely vague. Broca originally defined the inion as the median intersection point of the two *lineae nuchae superiores*. Merkel pointed out that the *protuberantia occipitalis externa* corresponded to the meeting point of two *lineae nuchae supremae*, and termed the meeting point of the *lineae nuchae superiores* the *tuberculum linearum*. Further the inion is often prolonged with a V-shaped projection, and in this case we are told that the median point of the base of this projection is to be considered as the inion, or that the *lineae nuchae* are to be continued on the same *niveau* until a median point is reached, which is to be the inion*. Now we shall term the median point fixed by the *lineae supremae* the *superior inion*, and the point fixed by the *lineae superiores* the *inferior inion*. If these two points coincide, as they may do, then this is the inion proper. Now Schwalbe defines the inion as the common inion of the four *lineae* when they are concurrent, and as the superior inion when they are not. This will work fairly well on many skulls, but our experience shows that on certain skulls, (i) only the inferior inion can be fixed,

* See especially G. Schwalbe: "Studien über Pithecanthropus erectus," *Zeitschrift für Morphologie und Anthropologie*, Vol. I. p. 24, 1899.

(ii) the two inions can both be fixed, but the inferior may be so far more marked than the superior that without question it would be treated as the inion on the living head, (iii) no inion at all can be determined with any certainty. Testing the living head one may say that if an inion can be fixed, one is not definitely certain whether it is the superior or inferior unless *both* can be determined. That more often than on the skull itself a cautious observer would decline to make any dogmatic statement as to the position of the inion at all. In fact on the skull there is often grave doubt, on the living head there is usually doubt and often gravest doubt as to where the inion should be located; and an occipital *torus* or a bathrocephalic network of ossicles, both not uncommon in English skulls, may completely put out any seeker for the inion on the living head. In short while it is highly undesirable to use the inion instead of the opisthion in any cranial investigation, the introduction of the inion into measurements on the living head is even more to be deprecated.

To illustrate these points we have investigated the inion on an important series (L.S.) of 116 English skulls. There were 50 ♂ and 66 ♀. Among these, owing to defects of one kind or another, 7 ♂ and 6 ♀ had no inion-opisthion distance measurable. The remaining crania we placed before Professor G. Thane and asked him to determine the position of the superior and inferior inions. He most kindly consented to give us all the advantages of his anatomical experience, and we have the following results:

Of the remaining 43 ♂ and 60 ♀ skulls the inion was "unrecognisable" or "undefinable" in 10 ♀ skulls, it was "very doubtful" in 1 ♂ and 2 ♀ skulls, and "vague" in 2 ♂ and 10 ♀ skulls; thus the ♂ skull gives usually a more marked inion than the ♀ skull. The superior inion was determinable more or less accurately in 41 ♂ and 43 ♀, the inferior in 28 ♂ and 23 ♀. In 6 ♀ and 2 ♂ cases the inferior inion *only* was distinguishable; only in 5 ♂ and 3 ♀ the two inions sensibly coincided. On the living head we very much doubt whether any precise inion could have been determined at all in about $\frac{1}{3}$ of the ♀ and $\frac{1}{5}$ to $\frac{1}{6}$ of the ♂ cases. A prominent point would have been taken, but whether it would be the inion at all, and if so whether it was the inferior or superior would not be determinable. The difference between the position of the two inions amounted at a maximum to 11 mm. in the ♂ and 18 mm. in the ♀; the average differences between them being 5 mm. in the ♂ and 8 mm. in the ♀.

The following are the results of measuring the inion-opisthion distance in mms.:

Inion	Male		Female	
	Mean	Standard Deviation	Mean	Standard Deviation
Superior Inion ...	47·0	4·7	47·2	5·6
Inferior Inion ...	42·3	—	39·2	—

Thus taking the probable error for the inion-opisthion distance in the case of the superior inion we see that half the ♂ population will have inions less than 45 or more than 50 mm., while half the female population will have inions less than 43 or more than 51 mm. from the opisthion. In the face of such a variability what can be said of Dr Beddoe's round 50 mm. for both sexes in his formula B_3 for the inion-opisthion arc? So far as English skulls are concerned he has not got even to the mean value of this arc. We can only protest strongly against a formula which involves such a rough approximation as this being used at all—as Dr Beddoe uses it—when dealing with the skull; it could only be used on the living head supposing there were a complete want of anything better; and this is certainly not the case.

We now turn to Dr Beddoe's transverse circumference, Q' in our notation; we have already seen that it is not the Q of the German or of our measurements, which are taken from the top of one auricular passage to the top of the other. This measurement is made at University College with the skull "horizontal" on the craniophor from the top of one ear-plug to the top of the other. The same point is vital in the consideration of the auricular height. Dr Beddoe without in the least troubling about this point remarks: "Le rayon pariétal de Barnard Davis était je pense identique à la hauteur auriculaire du Dr Lee*." The parietal radius of Barnard Davis differs sensibly from the auricular height of the *Verständigung*, and this error affects all Dr Beddoe's applications of Dr Lee's formulae to Barnard Davis' measurements. We consider that we should be scarcely wrong in allowing even as much as a quarter inch for the difference between the auricular height as measured on a craniophor and Barnard Davis' parietal radius from the centre line of the auricular passages to the *most prominent point of the parietal*.

This is probably the best that can be done, but it only shows how idle it is to attempt to use non-identical systems of measurements in any enquiry of this kind. We may take this opportunity of strongly protesting against the use of Barnard Davis' measurements at all for really scientific work. To begin with his capacities are certainly incorrect. Dr Beddoe reduces them to what he terms Flower's values by subtracting $\frac{1}{20}$, but we have strong reasons for believing that his errors in capacity measurement are not proportional, but to a considerable extent irregular. Further Barnard Davis tabulates his results only to $\frac{1}{10}$ of an inch and this is by no means sufficient for exactitude in craniological investigation. He appears also to have had curious views as to which tenth should be recorded†. In his tables of results for different races he gives in his first column the number of skulls used, but this number does not represent the number used in finding the mean of each character. Hence when the total number is small, as it usually is, no use ought to be made of Barnard Davis' mean results for calculating the mean capacity of

* *Loc. cit.* p. 271.

† For example, in finding means, where he also stops at the first decimal, he frequently (possibly invariably) records 4·29 as 4·2, and there is no evidence that his individual measurements are not treated in the same way.

the race by a formula. The presence or absence of one or two crania in one or other measurement entirely modifies the values. With our modern results for the continuous variation of skull characters within the race the manner in which Barnard Davis—and he appears to be followed by Dr Beddoe—picks the material upon which the average is to be struck is peculiarly annoying. A brachycephalic skull among a dolichocephalic series of three or four cannot be discarded in striking an average simply because it is brachycephalic! Yet this is the sort of line Barnard Davis adopts, and in very few cases is it possible to determine which skulls he has retained and which he has rejected in striking his average for any given character.

(7) From such material as this Dr Beddoe has largely drawn when testing his own formulae against Dr Lee's. He does not give us the information needful to determine whether he has struck fresh averages for himself or used Barnard Davis' averages. If he has used the latter, he seems unaware that he is frequently using means which do not correspond to the skulls used for the mean capacity, and that this is fatal to the validity of his inquiry in short series. In the next place Dr Beddoe has used no consistent or scientific measure of the goodness of the prediction. Generally he has taken the total range of error, which is of course absolutely fallacious. He ought to have used either the mean or mean square error, and to have fixed beforehand what was the standard capacity he proposed to reach. In the next place in using Dr Lee's formulae he has entirely disregarded her instructions as to the manner in which they were to be used. He has applied male formulae to find the capacity of female skulls, although Dr Lee has insisted on the sexual difference. He has used measurements which are not the measurements of her formulae to substitute in them. And lastly, and perhaps of most importance, is the fact that where we can test his arithmetic it is frequently quite incorrect. Surely the time has arrived when we must put aside all personal regard for a veteran and appeal in the interests of science for a totally different treatment and a totally different sense of responsibility when anthropometric problems are discussed.

(8) It is not our purpose in this paper to follow Dr Beddoe page by page and show the fallacy of his conclusions, but it is necessary to illustrate the general character of his methods. The following passage will almost in itself be sufficient:

"Les comparaisons suivantes ont trait à trois crânes de Naqada spécialement cités par Miss Fawcett (p. 20 *op. cit.**).

Manouvrier	B_1	Fawcett	B_3	B_2	P et L	Thompson	Thane
1408	1371	1343	1332	1322	1294	1289	1270

Ici mes procédés nous conduisent à des résultats plus corrects qu'aucun des autres. Je crois que ma méthode est digne d'être placée à côté des autres pour déterminer la capacité cranienne. Je vais maintenant l'appliquer, avec les modifications nécessaires, à la capacité des têtes vivantes†."

* The citation is incorrect. It should be *Biometrika*, Vol. I. p. 420. † *Loc. cit.* p. 283.

Now as B_1 and B_2 are formulae destined to give Barnard Davis' capacity and not the true capacity they need not be considered here. What we have to compare are Fawcett's actually measured capacity, Dr Beddoe's B_3 and "P et L."

Now looking up C. D. Fawcett's paper it will be found that the three skulls referred to have capacities of 1217, 1497 and 1222, which have a mean value of 1312 and *not* 1343 as Dr Beddoe calculates! Further two out of the three skulls are ♀ and Dr Beddoe applies the ♂ formula to them to get his "P et L" prediction! Lastly Dr Beddoe entirely disregards Dr Lee's instruction to apply the formulae for the most closely allied race, if it be known*. As Dr Lee has provided a formula for Naqada crania, Dr Beddoe ought to have used this; he would then have found for the predicted value of the three crania 1218, 1522 and 1197, giving a mean of 1312, differing by no single point and not by 19 as B_3 does from the observed result. Let us however compare the results for the three crania as found by B_3, not with the special Naqada formulae, but with Dr Lee's general formulae, paying, of course, the proper attention to sex.

TABLE III.

Naqada Skull	Capacity Measured	P and L Predicted		B_3 Predicted
		Naqada	General	
1308 ♀	1217	1218 (− 1)	1195 (−22)	1154 (− 63)
B 24^b ♂	1497	1522 (−25)	1507 (+10)	1602 (+105)
B 21 ♀	1222	1197 (+25)	1175 (−47)	1182 (− 40)
Mean ...	1312	1312 (0)	1292 (−21)	1313 (+1)
Mean Error	—	17	26	69

Now supposing we have followed Dr Beddoe correctly his results were not only 30 cubic centimetres wrong in taking the average of Miss Fawcett's three skulls, but 19 cubic centimetres wrong in using his own formula! This brings his predicted mean into close accord with Miss Fawcett's observed mean result. But we see at once on examining the three individual predictions that his success is only achieved by the balancing of large individual errors. His mean error is four times as bad as the P and L Naqada prediction and nearly three times as bad as the P and L general formula prediction. Allowing for faults of arithmetic, results similar to these must have been actually before Dr Beddoe when he wrote:

"Mes procédés nous conduisent à des résultats plus corrects qu'aucun des autres," etc.

* The University College anthropometric workers have again and again insisted that regression formulae vary from race to race. See *Phil. Trans.* Vol. 196, A, pp. 243 *et seq.*; *Biometrika*, Vol. I. pp. 460–1; Vol. II. pp. 347 *et seq.*

Hence as he certainly would not have screened them behind a mean value, we are forced to the conclusion that he really did not grasp their significance, or in other words that he is quite unfitted to deal with such statistical enquiries. But Dr Beddoe's agreement in the matter of the mean of the three skulls when his arithmetic is corrected is not only fallacious because it depends upon large individual errors of opposite sign but because he has used the *wrong values* of the auricular circumferences. To obtain his Q' he ought to have added at least 10 mm. to Miss Fawcett's Q. If this be done, his values for the three capacities are 1195, 1613 and 1232 respectively, giving a mean of 1347 and a mean error of 49,—results markedly worse than P and L. We do not know whether Dr Beddoe would prefer to somewhat reduce his mean error at the expense of his mean, or to get a good mean with an excessive mean error. As he uses our Q for his own transverse circumference, we must suppose the latter to be the case and shall not attempt to modify this arc for the remainder of this paper when applying his B_3.

In concluding this section we may note the results obtained by using our new circumferential formulae. We have applied the English ♀ formula* to the female skulls and the mean ♂ formula and the Naqada ♂ formula to the male skull. The following are the results:

TABLE IV.

Naqada Skull	Capacity Measured	Mean ♂ and English ♀	Naqada ♂, English ♀
1308 ♀	1217	1203 (−14)	1203 (−14)
B 24[b] ♂	1497	1542 (+45)	1527 (+30)
B 21 ♀	1222	1230 (+ 8)	1230 (+ 8)
Mean ...	1312	1325 (+13)	1320 (+ 8)
Mean Error	—	22	17

Thus the method of correlation, whether we use diameters or circumferences, gives for these three skulls immeasurably better results than Dr Beddoe's B_3; yet it is as a corollary to his results for these three crania that he writes:

"Je crois que ma méthode est digne d'être placée à côté des autres pour déterminer la capacité cranienne."

(9) We have already given our reasons for dissenting from Dr Beddoe's methods of applying his own and the University College workers' formulae to his material, and also our objections to the use of Barnard Davis' material at all†. Still this material, allowing for the rough approximations which have to be made

* The only series for which the female data has so far been reduced.

† It is greatly to be desired that a complete system of modern measurements should be taken and published, not only on the Barnard Davis collection, but on the remainder of the splendid material in the possession of the Royal College of Surgeons.

to apply the formulae to it, shows in a sufficiently marked manner the superiority of the correlation formulae over Dr Beddoe's purely empirical guess-work. We propose to test this on two of Barnard Davis' series. We will first take a series of nine groups of skulls and predict their means by B_3, P and L, and $G.F.$, where $G.F.$ stands for the general male formula (f) for circumferences given on p. 371 of this paper. In order to apply our results we have not taken Barnard Davis' mean values, which we have seen are unreliable, but have worked out afresh the means for all the ♂ crania in each series for which the entire system of characters needed are given. These characters are (i) the capacity C, (ii) the maximum length L, (iii) the maximum breadth B, (iv) the auricular height H, (v) the cephalic index I, (vi) the horizontal circumference U, (vii) the sagittal circumference S, (viii) the transverse circumference Q. We have obtained Dr Beddoe's S' from S by deducting, as he asserts, 1·97 inches for the inion-opisthion arc, and his Q' from the intermastoid arc of Barnard Davis by subtracting 2·3 inches, as he does. We reach our auricular height from Barnard Davis' parietal radius by subtracting ·25 inches, and our Q from his intermastoid arc by subtracting 2·5 inches. Only in the case of the Thais which number only four crania have we calculated the capacity of each individual skull and taken the mean result; in all the other cases the formulae had been used on the mean values of the characters. The measurements are in inches except C and I.

TABLE V.

Mean Values of Characters in 9 Races. ♂ Crania.

Race	No.	C in ozs.	L	B	H	I	U	S	Q
Romans of Britain	8	73·3	7·3	5·7	4·4	77	21·0	14·8	12·1
Australians ...	16	67·6	7·3	5·3	4·3	72	20·3	14·7	11·6
New Hebridians...	9	72·1	7·1	5·1	4·5	73	20·0	14·8	11·8
Kanakas ...	66	77·2	7·0	5·6	4·8	79	20·3	14·9	12·8
Marquesans ...	17	78·9	7·1	5·6	4·7	78	20·5	14·8	12·5
Netherlanders ...	16	79·5	7·2	5·7	4·5	79	20·7	14·7	12·5
Finns	7	79·2	7·0	5·8	4·6	82	20·6	14·8	12·7
Russians	11	80·9	7·4	5·8	4·5	78	21·2	15·0	12·4
Thais 1	1	76·0	6·6	5·6	4·85	84	19·8	14·6	13·3
„ 2	1	81·0	7·4	5·4	4·75	73	20·5	15·3	12·6
„ 3	1	79·0	6·6	5·9	4·75	89	20·2	14·3	13·4
„ 4	1	78·0	6·8	5·5	4·75	80	19·7	14·9	12·5

Dr Beddoe's S' and Q' are to be found from the above tabled values by subtracting 2 inches* from S and adding ·2 inches to Q respectively. The reader may well ask what is the value of such an attempt to reach indirectly quantities directly measurable on the skull? We should reply: Probably no value at all! We have seen that the inion-opisthion arc is extremely variable and the same

* 50 mm. = 1″·97, but as Davis' values are given to $\frac{1}{10}$ inch only the $\frac{3}{100}$ cannot be considered.

statement must be made with regard to the size of the mastoid processes. To assert that the mean value of the difference between the intermastoid arc and the bi-auricular arc is 2·3 inches is in our opinion wild guesswork, and even if it were true it neglects entirely the immense variability of this difference. We cannot expect any formula to give good results under such treatment, but as Dr Beddoe has chosen such a test we consent to accept it, especially as it enables us to indicate to the craniological reader the sort of statistico-metrical treatment which seems to have passed hitherto as science. In the following table we give first the supposed measured value of the capacity, i.e. Barnard Davis' values less 1/20 and reduced to cm.^{3}; then we give the predicted values and differences of B_3, P and L, and $G.F.$, and finally the mean errors of the three formulae.

TABLE VI.

Comparison of Cranial Formulae on Barnard Davis' Measurement of Mean Racial Capacity.

Race	Capacity	b_3	Δ	P and L	Δ	$G.F.$	Δ
Romans of Britain...	1385	1490	+105	1431	+46	1463	+78
Australians ...	1277	1347	+ 70	1330	+53	1366	+89
New Hebridians ...	1362	1366	+ 4	1309	−53	1376	+14
Kanakas	1459	1544	+ 85	1462	+ 3	1499	+40
Marquesans ...	1491	1507	+ 16	1454	−37	1473	−18
Netherlanders ...	1502	1515	+ 13	1441	−61	1477	−25
Finns	1497	1559	+ 62	1454	−43	1499	+ 2
Russians	1529	1570	+ 41	1492	−37	1522	− 7
Thais	1483	1539	+ 56	1430	−53	1487	+ 4
Mean	1443	1493	+ 50	1416	−27	1462	+19
Mean Error ...	—	—	50	—	43	—	31

In deducing the results P and L we have, owing to the mixture of races involved, used Dr Lee's most appropriate formula (10) *bis*, that in the footnote on p. 247 of her memoir*, based on the best results for the *means* of 10 races. It will be seen at once that P and L and $G.F.$ present much better results than B_3 which is far worse than either in determining the mean value of the whole series, and is worse than our formula by at least 50 per cent. in the mean error.

If Dr Beddoe considers that his formula has given all plus errors and therefore can be approximately bettered by reduction in his multiplier, we must call his

* *Phil. Trans.* Vol. 196, A, p. 247. We have used this formula throughout for race means because it is based on racial *means* and not on individual skulls. Experience has shown that in the long run it works better than (10). The latter is much better for this series, giving a mean value 1446, only three in excess, and a mean error of only 35! We have not, however, taken advantage of this superiority to use it.

attention to the following table, which is a reconstruction* of his Table III for the eight skulls out of his ten, for which we could find the requisite data:

TABLE VII.

Race	Capacity	B_3	Δ	*G. F.*	Δ
♂ Mountain Chinese .	1473	1423	− 50	1407	− 66
♂♀ Cossack	1530	1406	−124	1371	−159
♀ Hindu	1133	1050	− 83	1115	− 18
♂ Fatuhivan... ...	1720	1700	− 20	1538	−182
♀ Bodo	1644	1443	−201	1394	−250
♂ Australian ...	1313	1243	− 70	1292	− 21
♀ Papuan	1313	1228	− 85	1263	− 50
♂ Dutchman... ...	1644	1613	− 31	1543	−101
Mean Error ...	—	—	− 83	—	−106

Now B_3 is certainly better here than *G. F.* and the answer is perfectly simple if the reader will turn to the diagram on p. 375. For small skulls with more than the average capacity our result must be too small, but better than Dr Beddoe's; for large skulls with more than the average capacity Dr Beddoe's will give a better result than the line of mean values. In the above table Dr Beddoe has only taken three small skulls with more than the average capacity and we are naturally in all those cases better than he is. In the other cases of big skulls he is better than we are, and roughly better as the skulls are bigger. If he had only had a majority of small skulls with big capacities the tables would have been turned upon him. The fact is that nothing can be determined at all by Dr Beddoe's Tables II and III for 'thick' and 'thin' crania; by judicious selection of small thin skulls and big thick skulls we can make B_3 give execrable results as compared to *G. F.* or *P* and *L*, the selection being made before the predictions have been calculated. It all depends upon looking at the diagram on p. 375 and choosing skulls which fall on the other side of the Beddoe line to the regression line. What is quite clear from B_3 in Table VII is that Dr Beddoe cannot improve the results given in Table VI without making his position in Table VII much worse; i.e. he must sacrifice his claim to be better than us on *selected* individual crania. For whereas B_3 gives far too big values on Table VI, it gives far too small values on Table VII. To really test the correlation formulae against the guesswork formula of Dr Beddoe, the individual skulls must be sampled *at random* and not *selected*. To do this we started at p. 20 of Barnard Davis' *Thesaurus* and extracted the first male *undeformed* skull with the necessary data on this page, or if there happened to be none on this page, the nearest male skull on the preceding page or pages. We then passed

* Dr Beddoe's Table exhibits a number of misprints or arithmetical slips, and is invalid as far as *P* and *L* is concerned, because he has used male formula for female crania, and an erroneous value for the auricular height.

to pp. 35, 40, 55, etc. in order, repeating the process until 20 skulls had been extracted and the following table resulted. To this we applied P and L^*, $G.F.$ and B_3, with the results in Table IX.

TABLE VIII.

Individual Crania Sampled at Random from Barnard Davis.

Race	Barnard Davis' Number	C†	L	B	H	I	U	S	Q	S'	Q'
(1) Ancient Roman ...	171 (p. 18)	70·0	7·2	5·6	4·25	76	20·7	14·5	11·7	12·5	11·9
(2) Anglo-Saxon ...	674 (p. 34)	75·0	7·4	5·5	4·75	74	20·8	14·5	12·4	12·5	12·6
(3) English	1029 (p. 48)	94·0	8·0	6·3	5·25	78	22·8	16·6	14·4	14·6	14·6
(4) Scottish Highlander	176* (p. 64)	71·0	6·8	5·5	4·25	80	19·7	13·7	11·8	11·7	12·0
(5) French	1312 (p. 80)	74·5	6·9	5·4	4·25	78	20·0	14·1	11·6	12·1	11·8
(6) Italian	1178 (p. 95)	73·0	7·1	5·2	4·35	73	20·2	14·6	11·7	12·6	11·9
(7) Dutch Jew ...	1328 (p. 110)	79·5	7·5	5·5	4·35	73	21·2	15·0	12·0	13·0	12·2
(8) Russian Tartar ...	941 (p. 125)	76·5	7·0	5·7	4·45	81	20·6	14·6	12·0	12·6	12·2
(9) Hindoo	487 (p. 140)	69·0	6·8	5·5	4·55	80	19·5	14·3	12·3	12·3	12·5
(10) Gond	808 (p. 155)	65·5	6·8	4·9	4·45	72	18·8	14·1	11·7	12·1	11·9
(11) Bodo	735 (p. 169)	68·0	6·8	5·5	4·55	80	19·6	14·2	12·5	12·2	12·7
(12) Egyptian Arab ...	1015 (p. 185)	67·0	7·1	5·4	4·55	76	20·1	14·4	12·0	12·4	12·2
(13) Fantu	262 (p. 200)	67·0	6·9	5·2	4·55	75	19·3	14·4	12·1	12·4	12·3
(14) Kafir	586 (p. 215)	64·0	7·4	4·9	4·35	66	20·2	14·6	11·4	12·6	11·6
(15) Vancouver Islander	1211 (p. 229)	85·0	7·5	5·7	4·65	76	21·0	15·4	12·7	13·4	12·9
(16) Aymara	1198 (p. 244)	82·0	7·1	5·6	4·75	79	20·5	14·8	12·6	12·8	12·8
(17) Australian ...	99 (p. 260)	69·0	7·0	5·6	4·35	80	20·0	14·3	12·0	12·3	12·2
(18) Sumatran	1367 (p. 275)	67·0	7·0	5·1	4·35	72	19·8	14·0	11·4	12·0	11·6
(19) Dyak	280 (p. 290)	60·0	6·6	4·9	4·65	74	18·7	13·7	10·7	11·7	10·9
(20) Papuan	1401 (p. 305)	73·0	6·5	5·4	4·45	83	19·4	13·5	12·0	11·5	12·2

Here again we reach the same general conclusions, namely the correlation formulae give not only better means for the whole series, but less mean errors, and this, although a whole series of assumptions has to be made to get any results from them at all, if Barnard Davis' material be used. The further point has to be borne in mind that B_3 was 'guessed' to fit Barnard Davis' material, whereas the correlation formulae were calculated from data measured in an entirely different, and we consider, in a far more correct manner‡. Any mathematician using a formula of Dr Beddoe's type could get immeasurably better results by least squares or by correlation from Barnard Davis' data, but the labour would be wasted, for the formula would only apply to the very doubtful measurements of the *Thesaurus Craniorum* and not to the more reliable measurements now made in both Germany and England.

* If L, B, H be measured in inches the formula to give C in cubic centimetres is nearly for males: $C = 406{\cdot}01 + 5{\cdot}52214\, L \times B \times H$. This is the mean formula of Dr Lee for finding the capacity of *individual* crania.

† Ozs. of Calais sand. To be multiplied by Topinard's factor 19·89 to reduce to cubic centimetres and then by 19/20 to reduce to cranial capacities as now measured. The other measurements are in English inches.

‡ We believe the rise in the mean error from 47 to 64 when we pass from suitably measured crania (p. 371) to Barnard Davis' material is almost entirely due to the vagueness of the allowances to be made and the crudeness of Barnard Davis' measurements.

TABLE IX.

Table of Capacities of 20 Individual Crania Sampled at Random from Barnard Davis.

Individual	Capacity	B_3	Δ	*P & L*	Δ	*G. F.*	Δ
(1) Ancient Roman ...	1323	1383	+ 60	1352	+ 29	1383	+ 60
(2) Anglo-Saxon ...	1417	1461	+ 44	1474	+ 57	1457	+ 40
(3) English	1776	2197	+421	1867	+ 91	2011	+235
(4) Scottish Highlander	1342	1259	− 83	1284	− 58	1276	− 66
(5) French	1408	1291	−117	1280	−128	1305	−103
(6) Italian	1379	1347	− 32	1293	− 86	1362	− 17
(7) Dutch Jew ...	1502	1494	− 8	1397	−105	1481	− 21
(8) Russian Tartar ...	1446	1446	0	1386	− 60	1414	− 32
(9) Hindoo	1304	1365	+ 61	1346	+ 42	1356	+ 52
(10) Gond	1238	1199	− 39	1225	− 13	1249	+ 11
(11) Bodo	1285	1382	+ 97	1346	+ 61	1372	+ 87
(12) Egyptian Arab ...	1266	1366	+100	1369	+103	1370	+104
(13) Fantu	1266	1318	+ 52	1308	+ 42	1325	+ 59
(14) Kafir	1209	1281	+ 72	1277	+ 68	1334	+125
(15) Vancouver Islander	1606	1630	+ 24	1504	−102	1575	− 31
(16) Aymara	1549	1524	− 25	1449	−100	1483	− 66
(17) Australian ...	1303	1366	+ 63	1348	+ 45	1356	+ 53
(18) Sumatran	1266	1222	− 44	1264	− 2	1268	+ 2
(19) Dyak	1133	1063	− 70	1236	+103	1133	0
(20) Papuan	1379	1251	−128	1269	−110	1263	−116
Mean	1370	1392	+ 22	1364	− 6	1389	+ 19
Mean Error ...	—	—	77	—	70	—	64

(10) We next turn to measurements made on crania on the basis of the Frankfurt *Verständigung*, for which our formulae have been calculated. Here our difficulty lies with deducing Dr Beddoe's values, but as he expresses satisfaction with the fit of his formula for the Naqada skulls, using our Q, we will continue to use that quantity for his auricular arc Q'. We do not suppose any correction

TABLE X.

Races Measured according to Frankfurt Verständigung.

Race (♂ s)	*C*	*L*	*B*	*H*	*I*	*U*	*S*	*Q*	*S'*
Altbaierisch (78)	1503	180·6	150·5	120·7	83·3	524·3	365·1	329·7	315·1
English (72) ...	1477	189·1	140·7	114·6	74·4	524·3	377·1	307·9	327·1
French (56) ...	1473	180·0	143·4	112·9	79·8	518·2	366·0	312·2	316·0
Aino (76) ...	1462	185·8	141·2	119·3	76·5	522·5	372·8	328·5	322·8
Etruscan (78) ...	1454	182·9	143·5	115·9	78·5	522·8	376·0	317·0	326·0
Malay (39) ...	1424	174·3	142·4	116·9	81·9	505·2	365·3	320·8	315·3
Theban (164) ...	1388	181·9	136·6	114·3	74·8	510·8	372·4	306·1	322·4
Naqada (98) ...	1381	185·1	134·9	115·6	73·0	511·0	373·0	304·2	323·0
Copt (59) ...	1356	179·1	136·5	115·4	77·3	501·8	365·9	311·8	315·9
Negro (39) ...	1348	182·8	133·2	115·0	72·9	508·5	367·7	306·8	317·7

would be really satisfactory, and least of all to him. We use, as before, Dr Lee's inter-racial formula from the footnote, p. 247 of her memoir, i.e.

$$C = 321{\cdot}16 + {\cdot}000{,}370L \times B \times H.$$

The following results were obtained by using the three prediction formulae.

TABLE XI.

Predicted Capacities of 10 *Races.*

Race	C	B_3	Δ	P and L	Δ	$G.F.$	Δ
Altbaierisch ...	1503	1530	+27	1535	+32	1493	−10
English ...	1477	1439	−38	1449	−28	1448	−29
French ...	1473	1419	−54	1399	−74	1415	−58
Aino	1462	1521	+59	1479	+17	1509	+47
Etruscan ...	1454	1494	+40	1444	−10	1477	+23
Malay	1424	1428	+ 4	1395	−29	1415	− 9
Theban ...	1388	1376	−12	1372	−16	1397	+ 9
Naqada ...	1381	1362	−19	1389	+ 8	1396	+15
Copt	1356	1361	+ 5	1366	+10	1377	+21
Negro	1348	1344	− 4	1357	+ 9	1379	+31
Mean ...	1427	1427	0	1419	−8	1431	+4
Mean Error ...	—	—	27·2	—	23·3	—	25·2

Again both the correlation formulae come out with a less mean error than Dr Beddoe's B_3, the diametral formula being sensibly the best. For the means Dr Beddoe is slightly better. We may stay here to note that $G.F.$ is really a mean individual and not an inter-racial formula. The inter-racial formula for these ten races gives us:

$$C = 316{\cdot}2 + {\cdot}01852\ U \times S \times Q.$$

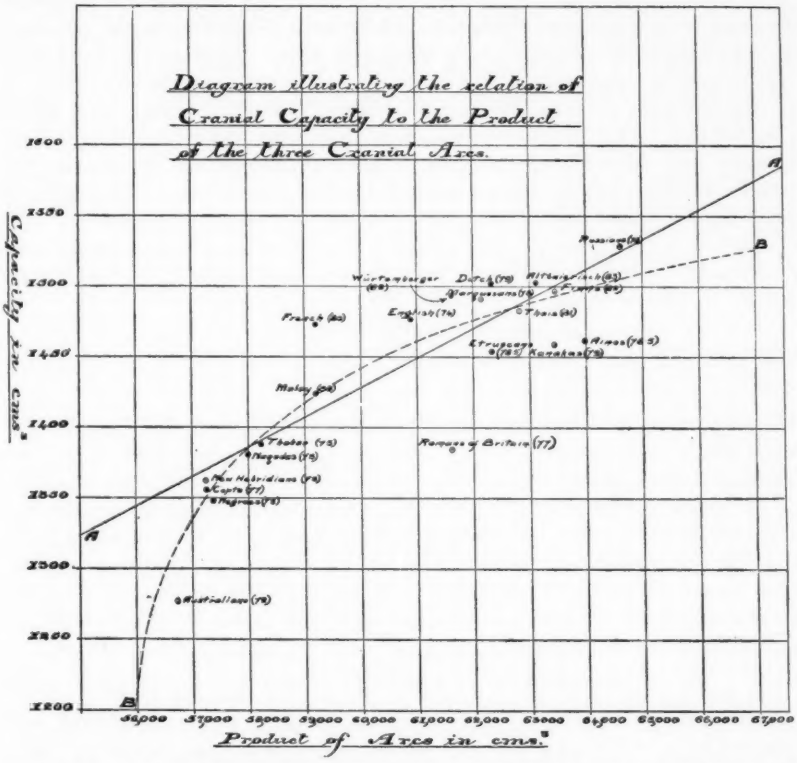
This formula should be used in preference to $G.F.$ for race means. It does not however give any very substantial advantage in the present case.

We have for the inter-racial arc formula ($I.A.F.$)

TABLE XII.

Race	Observed	$I.A.F.$	Δ	Race	Observed	$I.A.F.$	Δ
Altbaierisch .	1503	1485	−18	Theban ...	1388	1394	+ 6
English ...	1477	1444	−33	Naqada ...	1381	1390	+ 9
French ...	1473	1413	−60	Copt ...	1356	1377	+21
Aino ...	1462	1501	+39	Negro ...	1348	1379	+31
Etruscan ...	1454	1476	+22				
Malay ...	1424	1413	−11	Mean ...	1427	1427	0
				Mean Error	—	—	25·0

We have in this formula the mean value equal to that of the observed races, but the mean error is only reduced ·2, in other words our *G. F.* is not substantially bettered, it is as good a fit as can be got for this series from a *linear* formula. But this best fitting line is interesting from another point of view, which is exhibited in the accompanying diagram. We have plotted here the observed capacities of 19 races, the above 10 races and the 9 doubtfully measured races from Barnard Davis; the horizontal abscissa is the product in cubic centimetres of the three arcs U, S and Q. We have added the cephalic indices. The straight line AA is the inter-racial regression line based on the 10 races. We see that with the



exceptions of the Malays and the Marquesans*, both fairly close to the inter-racial line AA, all the other races above the line are *modern Europeans*. In other words it would appear that the modern European races gain their advantage in skull capacity over the lower or more primitive races largely by thinner bone. Of the races

* "Without exception the finest race of people in this sea. For fine shape and regular features they perhaps surpass all other nations." Cook, *Voyage towards the South Pole*, 1777, Vol. I. p. 308.

above the best fitting line Dutch, English, Russians, Marquesans, will have their values as calculated altered *for the worse* by Dr Beddoe's correction for cephalic index, the French will remain unaltered (although they want most correction!) and the Altbaierisch and Malay will be slightly improved (although they stand least in need of correction!). Of the races below the line all but Finns would be improved by correction although in several cases they would be over corrected. But a very slight inspection shows that the corrections needed are in no way proportional to the defect of their cephalic indices from 80. Thus omitting Australians, the Ainos, Kanakas, Etruscans and Romans of Britain* (all over 75) want most correction; while Thebans, Naqadas, New Hebridians (all under 75) want far less correction. In fact we consider that if Dr Beddoe had formed a similar diagram he would not have laid any stress on his cephalic index correction. What does seem to us to follow from this diagram is the non-linearity of the inter-racial regression line. It is very markedly curved. We have therefore endeavoured to correct by aid of a curved line. Until more data are available, measured and reduced in a uniform manner, it is not worth spending much labour in calculating the constants of possible formulae, but we may roughly point out how we get far better results by correcting for non-linearity of the regression curve for inter-racial results than by attempting to "compensate" by cephalic index. Two hypotheses occurred to us, (i) the regression line instead of being $C = a_1 + A_2 P$, where P is the product $U \times S \times Q$, may be taken

$$C = a_1 + A_2 P + a_3 P^2$$

or as parabolic, (ii) the regression line may be taken as logarithmic

$$P + a_4 = a_5 e^{a_6 C}.$$

The only reason, and it is very slight, for the latter form is that it makes the increment of C not only directly proportional to the increment of P, but inversely proportional to the absolute magnitude of P measured from a certain value. A very similar curve has been found to suit the change in human physical measurements with age. The curve is here used merely as a form judged likely from the observations, and without at present any statement of a theoretical basis being suggested. We have found roughly the constants from the 10 races available. There result the two formulae, P being in cubic centimetres:

$$C = -15319{\cdot}6 + {\cdot}531{,}942P \qquad - 4{\cdot}2075P^2/10^6$$
$$C = 704{\cdot}04 \qquad + 203{\cdot}05 \log_{10}(P - 55682).$$

The disadvantage of the first formula is that with the constants selected, one reaches a value of P after which the capacity begins to decrease, but a very little change of the constants would suffice to throw this value up to, say 1550 cms.^{3}, a value exceeding any *racial* mean† hitherto observed. The logarithmic formula

* We are inclined to doubt entirely the *data* for the Romans in Britain. It differs so widely from Romans elsewhere. We hope that these skulls may be remeasured shortly.

† We ask the reader to particularly note the words "*racial* mean," for our formulae are not to be transferred without further consideration to the calculation of individual capacities (or indeed to the female sex).

we consider preferable as it suffers from no such defect. Table XIII gives the results, compared with Dr Beddoe's compensation formula. We see at once that either of our formulae based on the obvious fact of non-linearity is far superior to

TABLE XIII.

Race	$U \times S \times Q$	Capacity	B_3	Δ	Parabolic Formula	Δ	Logarithmic Formula	Δ
Altbaierisch ...	63118	1503	1530	+ 27	1493	−10	1490	−13
English	60878	1477	1439	− 38	1470	− 7	1459	−18
French	59206	1473	1419	− 54	1428	−45	1424	−49
Aino	63988	1462	1521	+ 59	1491	+29	1500	+38
Etruscan ...	62327	1454	1494	+ 40	1490	+36	1480	+26
Malay	59197	1424	1428	+ 4	1425	+ 1	1424	0
Theban	58223	1388	1376	− 12	1389	+ 1	1395	+ 7
Naqada	57991	1381	1362	− 19	1379	− 2	1387	+ 6
Copt	57254	1356	1361	+ 5	1344	−12	1353	− 3
Negro	57373	1348	1344	− 4	1350	+ 2	1359	+13
Mean	59955·5	1427	1427	0	1426	− 1	1427	0
Mean Error ...	—	—	—	27·2	—	14·5	—	17·3
Romans in Britain	61609	1385	1490	+105	1482	+97	1470	+85
Australians ...	56722	1277	1347	+ 70	1316	+39	1317	+40
New Hebridians	57234	1362	1366	+ 4	1343	−19	1352	−10
Kanakas	63441	1459	1544	+ 85	1493	+34	1494	+35
Marquesans ...	62145	1491	1507	+ 16	1489	− 2	1478	−13
Dutch	62327	1502	1515	+ 13	1490	−12	1480	−22
Finns	63447	1497	1559	+ 62	1493	− 4	1494	− 3
Russians ...	64614	1529	1570	+ 41	1485	−44	1506	−23
Thais	62827	1483	1539	+ 56	1493	+10	1487	+ 4
Mean	61596	1443	1493	+ 50	1454	+11	1453	+10
Mean Error ...	—	—	—	50	—	29	—	26
Mean of 19 Races	60733	1434	1458	+ 24	1439	+ 5	1439	+ 5
Mean Error ...	—	—	—	38	—	21	—	21

his B_3, not only in the mean of each series, but in the mean error. In fact in only two cases out of the 19 races is he better than the parabolic and in only three cases better than the logarithmic formula. Thus in his Barnard Davis results he is nearly 100 per cent. worse than the logarithmic formula. Nor can he better himself by changing his multiplier because he has already got a zero deviation from the mean of his series in the *Verständigung* measurements, i.e. the far better series. So soon as material is reduced on a uniform basis for inter-racial data, we believe it will be possible to adjust the constants of a non-linear inter-racial regression formula to read with a mean error of under 14 cms.[3]. We do not attempt this at present, because the bulk of sound material is insufficient.

(11) We now turn to our last test, that on individual crania measured on the basis of the Frankfurt Concordat. We have taken 45 ♂ skulls by sampling at

random* C. Fawcett's Naqada, T. Ranke's 'Altbaierisch' and E. Schmidt's Theban mummy measurements. We have applied Dr Lee's mean formula for individual crania†. B_3 and the general formula were used as in the series of Table XI. It is impossible to print all the measurements and results for these 45 skulls but the general conclusions are given in the table below:

TABLE XIV.

Results of the Three Formulae for 45 ♂ Crania.

Series	Quantity	B_3	P and L	*G. F.*
Naqada Series ...	Mean Error	64	50	51
Altbaierisch Series ...	Mean Error	50	62	49
Theban Series ...	Mean Error	49	46	42
Whole Series	Mean Error	54	53	47
	Range of Error	+245 to − 169	+141 to − 164	+148 to − 165

For a brachycephalic race as we see from the table Dr Beddoe gains by his cephalic index correction and this is the source of his advantage over P and L for the Altbaierisch. But for a dolichocephalic race like the Naqada or Theban he loses; thus his biggest negative error arises in the case of the largest Naqada skull where his correction for cephalic index still further reduces his underestimate.

On the whole again we see that the advantage lies with the correlation formulae. Summing up our results we should say that for the skull:

(i) Intra-racial and inter-racial formulae can be found for circumferential measurements which are superior, or at any rate equal to Dr Lee's diametral formulae.

(ii) Such formulae give better results when their constants are found by statistical theory than by mere guesswork.

(iii) The vagueness of the inion as a point of measurement renders it of the greatest importance that the sagittal arc should be measured from the opisthion and not from the inion.

(iv) It is conceivable that a double regression formula would work better than a single regression formula like P and L or our present *G. F.* But it will hardly be obtained by taking as the second variable the product $I \times U \times Q \times S$.

(v) Probably the best inter-racial results will be obtained by non-linear regression formulae based on a single product.

* For the mummies every tenth skull starting with No. 489 of the Catalogue was taken; for the Naqada every tenth skull so far as possible, allowing for the mixture of ♂ and ♀ on the list; for the Altbaierisch,' every fifth as far as the requisite data were provided.

† $C = \cdot 000337\ L \times B \times H + 406 \cdot 01$.

We hope later to publish double regression formulae for skull capacity, but if it is to be done by the use of an index, we do not believe that the cephalic index is the right character to use, because it has been shown to have small correlation with the capacity, and what correlation does exist changes sign from race to race. On the whole we believe that increased accuracy is to be sought as we have indicated on p. 388* by using non-linear regression rather than multiple regression formulae, unless indeed one variable of the latter be the thickness or the weight of the skull.

(12) Having dealt with the application of Dr Beddoe's formula to skulls, we now turn to its *raison d'être*, i.e. its applicability to the living head. We have already considered one grave objection, namely, that the inion is on the skull itself by no means a definite and easily located point. On the living head we find it even more difficult to determine it with certainty. By throwing the head forward in *certain* cases the boundary of the area to which the muscles are attached can be appreciated, but "well-marked inions," i.e. large bony growths in the neighbourhood of the inion, occipital ridges and toruses, especially frequent in English heads, render the exact localisation extremely problematical. Let us suppose, however, that the inion could be ascertained on the living head, then the measurement of the three arcs seems to us entirely valueless on account of the varying amount of hair. Dr Beddoe really makes no attempt to meet Topinard's objection to the use of these arcs on the living head, except by the remark that abundance and stiffness of hair is not usually met with in combination with intellectual eminence! He gives no statistics, however, of the relative baldness of the average middle-aged agriculturist, tradesman, or man of science. Without such statistics his remark is worthless. Having a bad case himself he simply drops it in order to point out the difficulties of rival formulae! Thus he writes†:

> Cette objection en ce qui concerne les cheveux s'applique aussi à la hauteur auriculaire et au maximum de longueur et largeur, lorsqu'ils sont pris au céphalomètre et non avec le compas d'épaisseur.

We do not know to what form of cephalometer Dr Beddoe is referring, but any anthropologist would be perfectly incompetent for his task who could not devise an instrument for reading the auricular height by a sufficiently blunt point so that the determination of the height would not be affected sensibly by the existence of the hair. You can clear the hair to one side when measuring the diameters, you cannot possibly get it out of the way when using the measuring tape. This is not mere theoretical reasoning, but the actual result of experience in practice. Next Dr Beddoe endeavours to discredit the allowance Dr Lee has made for the thickness of the flesh on the living head. It seems to have escaped him that if the varying thickness of the flesh is a real difficulty in determining the diameters, its effect on his own arcs (in addition to the effect of the hair!) can hardly be described as less than insuperable. But we are not inclined to give much weight

* *Phil. Trans.* Vol. 192, A, pp. 222 *et seq.*
† *Loc. cit.* p. 285.

to his objection. Comparing the mean diameters found from more than 100 English ♂ skulls with the means found on more than the same number of living ♂ heads we have differences of 9, 12, and 11 mm. in the case of maximum length, breadth and auricular height respectively. Dr Lee's allowance of 11 cms. seems therefore a very reasonable one, and is closer than it appears, for the heads were certainly—allowing for the difference of skull and head—more brachycephalic than the earlier skulls. We do not say that Dr Lee's method is beyond criticism; on the contrary we recognise all the inherent difficulties of the problem. But we believe these difficulties to be enormously exaggerated by Dr Beddoe, and that he screens behind this exaggeration the far greater difficulties associated with his own method of arc measurement.

(13) As to his application of that method to demonstrate the correlation of intelligence and skull capacity, we hold it to be quite fallacious. To begin with he selects a formula—by guesswork—which is theoretically incorrect. He drops his formula for the skull and takes the product of $\frac{1}{3}$ of each of his arcs, and divides this product by 2000. He then increases it by $\frac{1}{3}$ per cent. for every unit of the cephalic index above 50. In his cranial formula he made no correction for cephalic index when it was 80. He now drops the neutral index to 50 without any justification or explanation. As we have already seen that the correlation between cephalic index and the capacity of the English skull is *negative*, his formula will tend to exaggerate the capacity of the brachycephalic and lower the capacity of the dolichocephalic individual heads. Now we believe it to be a fact—whatever may be the explanation, lie it in environment, nurture or difference of race—that the well-to-do classes in this country are now more round-headed than the working classes*, but such are the classes which produce the bulk of the people who work intellectually, and therefore Dr Beddoe's erroneous allowance for brachycephaly directly tends to emphasise the cranial capacity of the intellectual classes.

Dr Beddoe says that according to Dr Lee there is no correlation between the development of the intelligence and the cranial capacity, and remarks†:

> Les matériaux sur lesquels cette dernière opinion est basée sont rares et peu concluants.

Dr Lee makes no such statement at all. What she asserts is‡:

"That there is *no marked* correlation between skull capacity and intellectual power."

Dr Beddoe forgets to draw attention to the fact that Dr Lee, in conjunction with ourselves, has actually measured the relation between size of head and intelligence, and on far more ample material than Dr Beddoe uses. We find the correlation sensible, but so small that it is impossible to base any prediction from the size of head as to general intelligence§. As to Dr Beddoe's own data bearing

* See *inter alia* Macdonell, *Biometrika*, Vol. I. p. 190.

† *Loc. cit.* p. 267.

‡ *Phil. Trans.* Vol. 196, A, p. 259.

§ *R. S. Proc.* Vol. 69, pp. 333—42 and Vol. 71, p. 106 *et seq.*

on intelligence and capacity, we can only repeat Dr Lee's words, which he would have done well to take to heart, namely that to solve a problem of this kind it is absolutely needful to keep within one "fairly equally nourished class," and if possible within one local race. We do not minimise the difficulty of doing this, but until it is done no final answer can be given to the problem. Much of the discussion at present might be summed up in such an absurd argument as: The Bavarians have as a race a larger skull capacity than the French; they are a less keen-witted race; ergo: smaller skull capacity goes with intelligence. We take it such an argument would be quite as valid as Dr Beddoe's comparison of Highlanders and East Anglians, or of Cornishmen of the upper and lower classes.

(14) As to whether Dr Beddoe's reconstructed capacities have any relation absolutely or relatively to the true capacities of the individuals he has measured it is of course impossible to say. But we are in the unique position of being able to test his formula on the head of one of the most distinguished Englishmen not only of his own time, but of all time. For width of view, logical clearness, and intellectual grasp, there were but few Englishmen in his own day, and there have been few since, whom we can consider as surpassing Jeremy Bentham. Bentham died in June, 1832, and a few months before his death he prepared a monograph which was printed in a few copies only, one of which is preserved in the British Museum. It is entitled "Auto-Icon or the uses of the Dead to the Living; a fragment from the MSS. of Jeremy Bentham." The object of this work was to show "how, if embalmed, every man might be his own statue*." Bentham left not only his manuscripts but his body to University College, then the University of London, and his *Auto-icon* is now preserved there. The head is in a mummified condition, but as the accompanying plates from our photographs will show, gives one a fair appreciation of Bentham in his old age. We still see the "venerable locks which floated over his collar and down his back"—in the manner of the great German historian—locks which convince one of the difficulty attending any measurements of the arcs on the living head. The flesh, if dried, is still there in considerable thickness, and this and the hair render the determination of the inion as difficult, if not more difficult than in the case of the head of the living. The opisthion and basion are exposed, and except for a couple of wires crossing the brain cavity, there is little or no matter left inside the skull; in fact its capacity can be determined as accurately as that of the majority of skulls with which one is called upon to deal. Measurements of this head were taken by one of us. There was a marked projection at 65 mm. from the opisthion, which was selected as the inion. But to be quite sure of the matter Professor Thane was consulted, and he fixed on practically the same point. There was, however, a lower point at 51 mm. from the opisthion, and between these two points a groove could be distinguished. Whether these two points correspond to a superior and inferior inion it would not, perhaps, be possible to determine, even if the flesh were removed. Anatomical authority spoke strongly for the upper point as inion. We will refer

* *Dict. Nat. Biography, sub* Jeremy Bentham.

to them as 'superior' and 'inferior' inions. We place here the values of the measurements taken on the Bentham head. Professor Thane with his usual kindness consented to take the critical arcs and diameters independently, and his measurements agreed within a millimetre with ours.

TABLE XV.

Measurements on the Head of Jeremy Bentham.

Auricular Height	121,	Less flesh	116,	Average	English	♂	115
Maximum Length	192	„	186	„	„		189
Maximum Breadth	153	„	147	„	„		141
Minimum Forehead Breadth	105	„	99	„	„		98
Bizygomatic Breadth	133?	„	127?	„	„		130
Horizontal Circumference	560	„	540?	„	„		524
Biauricular Arc	335	„	325?	„	„		308
Sagittal Arc to Opisthion	380	„	365?	„	„		377
„ „ 'Superior Inion' ...	315	„	300?	„	„		330
„ „ 'Inferior Inion' ...	330	„	315?	„	„		335
Opisthion to 'Superior Inion' ...	65	„	—	„	„		47
„ „ 'Inferior Inion' ...	51	„	—	„	„		42
Face Height	144?	„	—	„	„		—
Upper Face Height	73?	„	—	„	„		70
Total Height, Basion to Bregma ...	135	„	132	„	„		132
Skull Base, Nasion to Basion ...	105	„	102	„	„		102
Profile Length (Alveolar Point to Basion)	89?	„	—	„	„		96
Palate Length	47?	„	—	„	„		48
Palate Breadth	35?	„	—	„	„		37
Nasal Height	55?	„	—	„	„		51
Nasal Breadth	31??	„	—	„	„		24
Capacity	1475	„	—	„	„		1477
Profile Angle	93°?	„	—	„	„		86°
Basal Angle	44°·5	„	—	„	„		41°
Nasal Angle	58°	„	—	„	„		65°
Alveolar Angle	77°·5	„	—	„	„		73°
Cephalic Index	79·7	„	79·0	„	„		75·0
Height/Length	70·3	„	70·9	„	„		70·0
Auricular Height/Length	63·0	„	62·4	„	„		60·6
Face Height/Bizygomatic Breadth ...	108	„	—	„	„		—
Upper Face Height/Bizygomatic Breadth	55	„	57·5	„	„		54
Nasal Index	56??	„	—	„	„		48
Palate Index	74·5	„	—	„	„		76

We place these measurements here because they may be of interest for other than our immediate purpose, merely remarking that difficulty of access or other reasons render some very doubtful. We notice at once that Jeremy Bentham's head is closely identical with that of the mediocre or average Englishman. Apparent exceptions are the very arcs which Dr Beddoe measures, but the difference here is, we believe, solely due to the difficulty of allowing for the flesh, and to a less extent for the hair. The only real exception we believe to be the

profile angle, the face apparently receding from nasion to alveolar point. This receding character is undoubtedly exaggerated in the photograph, although great care was taken in the endeavour to get the 'horizontal plane' perpendicular to the vertical sides of the plate. Still, difficult as the determination of the lower rim of the orbit is in the present condition of the head, we believe the profile angle is truly greater than 90°, and that this is the case receives confirmation from the remarkable smallness of the nasal angle. With the exception of this receding character the head of this man of first-class intellect shows no single measurement—least of all its capacity—which would serve to differentiate it from that of the average Englishman of his time. Statistically, it is idle of course to argue from a single instance; but it is certainly worthy of note that Jeremy Bentham if judged by head-capacity would have been simply mediocre.

We have now to see what results the various reconstruction formulae as applied to Jeremy Bentham's measurements provide. We must note that all the measurements were taken and controlled by Professor Thane's independent measurements before any formulae were applied. To use Dr Lee's formula, the thickness of the flesh in its present condition at various points was tested by aid of a fine sharp needle. At the glabella it was 4 mm.; at the bregma 2 to 3 mm., and about the same over the back of the head. We accordingly took the cranial diameters to be $L = 186$, $B = 147$ and $H = 116$ respectively. Applying Dr Lee's formula

$$C = \cdot 000337 \times L \times B \times H + 406 \cdot 01,$$

we find: $$C = 1475 \text{ cm.}^3.$$

To apply the General Formula of the present paper we roughly allowed for the flesh on the basis of a whole circumference for the horizontal circumference, and a half circumference for the sagittal and auricular arcs, taking as our values 540, 325 and 365 mm. This must give a value slightly too big, as we have not allowed for the hair. There results 1511 cm.^{3}. Now the actual capacity is certainly slightly over the measured value 1475 and almost certainly under 1495 cm.^{3}. The exact agreement of P and L with the measured capacity is of course only pure chance. But either P and L or $G.F.$ would suffice to show that Bentham had skull capacity close to the average English value. P and L is of course the more satisfactory because we have made no allowance that was not capable of measurement.

We now turn to Dr Beddoe's formula. We are to take $\frac{1}{5}$ of each of the arcs, divide the product by 2000, and add ·3 per cent. for each point of the cephalic index, about 50.

In applying this rule we will give Dr Beddoe every advantage, which he in his turn has not always given to those whom he criticises. Our auricular arc is measured with the head on the craniophor from the *top* of one auricular passage to the other. From the *centre* of one auricular passage to the other appears to be 343 mm. The horizontal circumference is 560 mm. and the nasion to the

'superior' inion 315 mm. The cephalic index is 79·7 points, say 30 above 50. The answer is:

$$C = 1221 \text{ cm.}^3.$$

To give Dr Beddoe more grace, suppose, which is extremely improbable, that the 'inferior' inion is to be taken as the true inion. The sagittal arc is now 330 instead of 315 and the answer is:

$$C = 1280 \text{ cm.}^3.$$

But we will go even further. Dr Beddoe makes light of the hair when measuring his arcs and therefore he cannot consider of much importance the variation in the flesh. We will, however, endeavour to improve his position by allowing for the effect of shrinkage of the flesh on his circumferences, although we believe that this shrinkage is not comparable with the effect of different states of the hair. It can only be the roughest approximation. A circle of radius $r + 5{\cdot}5$ mm. has for circumference $2\pi\,(r + 5{\cdot}5)$ mm. Hence, if 5·5 mm. shrink to 3 mm. there would be a drop of $2\pi \times 2{\cdot}5$ mm. in the circumference, say 16 mm. Hence, at the outside we cannot allow more than 16 mm. for change in the horizontal circumference due to shrinkage, and possibly 10 mm. in the auricular arc, and say 13 in the sagittal. This gives us as measurements in life:

576, 353 and 328 for 'superior' inion,

576, 353 and 343 for 'inferior' inion.

The first and more probable inion gives us now:

$$C = 1346,$$

and the second and improbable inion:

$$C = 1408.$$

Dr Beddoe's estimate would then be at least 70 cm.^{3}, but most probably 150 to 200 in defect, in the case of Jeremy Bentham! This would not only place Bentham, undoubtedly as able as, if not abler than anybody on Dr Beddoe's list, at the very bottom of it, but a long way below the mediocre Englishmen with whom he is really identical. Thus, it seems to us, that in the only case wherein we have been able to test Dr Beddoe's formula and his hypothesis of marked correlation between skull capacity and ability both fail completely. This may be only the exception which proves the rule, but we must confess that it makes us entirely distrust not only his guesswork formula, but the deductions as to ability and skull capacity which he has based upon it.

(15) *Conclusion.* In concluding this paper we want particularly to emphasize one or two points. We are not defending a particular formula; we believe, owing to the results obtained, that a circumferential formula as good as a diametral formula can be reached. We are not fighting a particular group of workers; in particular we believe that Dr Beddoe has done good service in widening the field of anthropometric interest in this country. What we want to emphasize are the following principles:

Auto-icon of Jeremy Bentham.

Front View.

Auto-icon of Jeremy Bentham.

Profile.

(*a*) Statistical enquiry is not a field for guesswork and elementary arithmetic; there is a mathematical science of statistics which must be learnt, and papers dealing numerically with anthropometric and craniometric data which do not now apply this theory are simply outside the field of science.

(*b*) Method in statistical reduction is not the only thing we demand however. In the light of modern scientific enquiry we demand that the craniologist shall distinguish between what holds for a local race of man, and what may be applied to mankind as a whole. We have elsewhere shown* by actual measurements that inter-racial and intra-racial correlations are not the same, and consequently the reconstruction formula for the individual within a given race is not the same as the formula for reconstructing the mean of a given race.

Dr Beddoe draws no distinction here, just as he draws no distinction between the sexes, although Dr Lee gives every warning on this point. It is only when we find local race formulae closely in agreement among themselves that we can extend our results and form from them an inter-racial formula. Dr Beddoe makes no attempt to deduce intra-racial formulae from fairly homogeneous racial series by recognised statistical methods; he does not then compare these among themselves and see whether an inter-racial one can be deduced from them. He simply makes a guess, tries it on a most heterogeneous series, and if it does not fit makes another guess. He applies Dr Lee's results regardlessly to individuals and to race-means, he uses the same formula for male and female skulls, and where he has no data to offer, although by time and patience he could have collected some, he simply makes the roughest appreciation. This is not science; it is the dilettantism which in the past has made anthropometry and craniometry impossible subjects for academic study. We believe that the time has come to change this, and uphill as the battle will be we shall not hesitate to criticise in the strongest manner papers like Dr Beddoe's, which sensibly lower the already low standard of craniometric science.

* *Biometrika*, Vol. II. pp. 347—56.

ÉTUDE BIOMÉTRIQUE SUR LES VARIATIONS DE LA FLEUR ET SUR L'HÉTÉROSTYLIE DE *PULMONARIA OFFICINALIS* L.

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INTRODUCTION.

Ce travail est relatif à la Pulmonaire officinale. Nous avons l'intention de l'étendre à d'autres plantes à fleurs hétérostylées en vue de contribuer à la connaissance du phénomène intéressant de l'hétérostylie.

Plusieurs herborisations nous avaient montré, chez *Pulmonaria officinalis*, de grandes variations dans la longueur des styles de chacun des deux types brachystylé et dolichostylé. Fig. 1.

Nous nous sommes proposé

(1) D'étudier, par des mensurations nombreuses, la morphologie de la fleur et l'hétérostylie de *Pulmonaria officinalis* L., pour en dégager des données numé-

riques précises sur le type moyen de chaque organe, sa fixité relative, et l'étendue de ses variations possibles.

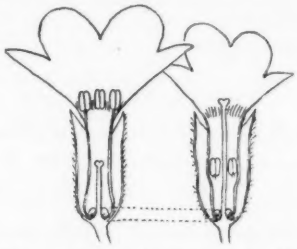


FIG. 1. *Pulmonaria officinalis* Gr. 2/1.
(d'après Hildebrand, Botanische Zeitung, 13 janv. 1865).

(2) De déterminer la nature des courbes, relatives à la variation de longueur des organes des deux types d'une fleur hétérostylée.

(3) D'établir si, en des localités différentes, le type floral subit des modifications de races orientées dans un sens déterminé, et si on retrouve le même type moyen et la même loi de fréquence.

Cette dernière question, on le voit, présente un certain intérêt au point de vue de la géographie botanique. C'est seulement à l'aide des études biométriques comparées, qu'il est possible de dégager, d'une part l'étendue de la variation possible d'une espèce dans les diverses localités de sa station habituelle, d'autre part de déterminer dans quel sens la variation a une tendance à s'accentuer.

Ces mesures biologiques ont en outre l'avantage de laisser des documents statistiques sur l'état d'une espèce qu'il peut être intéressant d'étudier à nouveau plus tard, au point de vue de son évolution possible.

Avant d'aborder ce travail je tiens à reconnaître combien j'apprécie l'honneur de le voir inséré en langue française dans *Biometrika*. Je veux aussi rendre un hommage de reconnaissante gratitude et de remerciements à M. le *Professeur W. F. R. Weldon*. Non content de revoir mon manuscrit, mon savant confrère a bien voulu m'aider de sa critique et de sa collaboration précieuse dans la tâche ingrate du calcul mathématique de nombreuses données numériques. C'est pour moi un devoir de le reconnaître ici et de l'en remercier chaleureusement.

Historique.

En réalité, nous possédons bien peu d'études de biométrie florale. Deux auteurs, Darwin* et Hildebrand†, pourtant, à propos des Pulmonaires, ont été amenés à dire quelques mots sur les différences florales observées entre les fleurs brévistylées ou brachystylées et les fleurs du type dolichostylé ou brévistémoné. Mais ce qui préoccupait ces auteurs c'était la question de la fécondité relative des

* Ch. Darwin, *Des différentes formes des fleurs dans les plantes de la même espèce*, trad. Heckel, p. 110. Reinwald, 1878.

† Hildebrand, *Die Geschlechter-Vertheilung bei den Pflanzen*, 1867, p. 37; *Botanische Zeitung*, 1865, 13 janvier, p. 13.

deux types de fleurs, et aucune statistique, ni aucun graphique n'ont appuyé leurs observations sur les dimensions des organes floraux.

Voici d'ailleurs les seules données signalées par eux :

Chez *Pulmonaria officinalis*, le pistil de la forme dolichostylée, dit Darwin, est deux fois aussi long que celui de la forme brachystylée, et les étamines diffèrent d'une manière correspondante quoique inverse.

Les grains polliniques sont différents dans les deux sortes. Ceux de la première forme sont à ceux de la seconde comme 78 est à 100 en longueur, 6 à 7 en épaisseur.

La corolle est plus grande généralement dans les fleurs brachystylées (et c'est l'inverse chez *Pulmonaria angustifolia*).

Hildebrand a ramassé dans le Siebengebirge 10 *Pulmonaria officinalis* de chaque sorte.

Les 10 dolichostylées ou brévistémonées portaient 289 fleurs, les 10 brachystylées ou brévistylées en portaient 373.

Hildebrand conclut de là que les sujets brachystylés produisent beaucoup plus de fleurs. La conclusion est basée comme on le voit sur un nombre très petit d'observations. Darwin reconnait d'ailleurs que 10 plantes anglaises dolichostylées, examinées par lui, furent très fécondes après fécondation illégitime, tandis que les 10 plantes semblables, récoltées en Allemagne par Hildebrand, restèrent complètement stériles. Cette différence semble indiquer une certaine variabilité de la fertilité.

Les plantes de Darwin cultivées en plein air ne montrèrent aucune tendance à devenir isostylées, et à perdre leur propre caractère dolichostylé, comme cela ce présente souvent parfois sous l'influence de la culture, dans plusieurs espèces hétérostylées de Primula.

Darwin signale la grande variété de longueur du pistil et des étamines de *Primula angustifolia*, et trouve que, chez cette plante, la distance entre les anthères et le stigmate n'est pas constante ; d'une moyenne de 7 mensurations seulement il conclut que cette distance dans la forme dolichostylée est à cette de la forme opposée comme 100 est à 69. Les longueurs relatives moyennes des pistils de ces deux formes seraient entre elles comme 100 est à 56, et pourraient atteindre même le rapport $\frac{100}{33}$. Cette variation si accentuée fait soupçonner à Darwin un état de transition et une tendance de la plante à devenir dioïque.

D'autre part Hildebrand signale que *Pulmonaria azurea* n'est point hétérostylée.

Les quelques mensurations, signalées ci-dessus, semblaient démontrer que l'hétérostylie était le mieux fixée chez *P. officinalis* pour diminuer de régularité chez *P. angustifolia* et disparaître chez *P. azurea*.

Généralités.

Prenons un nombre N d'échantillons de fleurs de Pulmonaires. Supposons qu'il s'agisse d'établir le type de fréquence et le type de longueur moyenne du style de ces fleurs.

Nous mesurons les styles. Ayant porté sur l'axe des abcisses les longueurs en millimètres exprimant la taille, nous élevons, aux divers points, des ordonnées de hauteurs proportionnelles aux nombres d'individus correspondants.

Lorsqu'on étudie comparativement des fleurs provenant de localités différentes il faut naturellement autant que possible prendre le même nombre d'échantillons de chaque localité. Une restriction pourtant doit être faite. Il y a souvent plus d'avantage à prendre le plus grand nombre possible.

La variation de longueur du style n'est pas discontinue. Dans nos mensurations nous avons ordinairement pris comme unité le $\frac{1}{2}$ millimètre. Vu le grand nombre d'échantillons à mesurer, il ne pouvait être question d'employer la loupe, il fallait donc choisir une unité facile à lire à l'œil nu avec une approximation suffisante. Mais, les abscisses ne croissent pas en réalité de $\frac{1}{2}$ en $\frac{1}{2}$ millimètres, mais bien de quantités infiniment petites, c'est-à-dire de x à $x + dx$.

En joignant le haut des ordonnées, le graphique que nous obtiendrons sera donc un polygone de variation (Pearson) qui tend seulement vers la courbe limite de ce polygone. Ainsi il y a intérêt à prendre un nombre le plus grand possible d'individus afin d'esquisser le mieux possible les vrais contours du polygone théorique artificiellement obtenu qui doit renseigner sur la courbe.

Pour donner une idée de l'approximation relative qu'il est possible d'obtenir avec *un petit nombre* de 25 exemplaires, nous donnons ci-après (fig. 2*a*) une courbe tracée avec deux lots de 25 plantes et celle qu'on obtient avec 31 plantes dont on a déterminé la distance du stigmate à l'anthère. Ces plantes ont été récoltées à des distances de 3 mètres au moins, au hasard des rencontres. Une fleur de chaque pied a été mensurée.

Plus le nombre des individus s'accroît, plus la courbe a une tendance à monter vers les ordonnées de grande fréquence, mais les parties latérales correspondant aux faibles fréquences restent partiellement indéterminées et s'étendent de plus en plus loin.

La courbe A (31 pl.) est enveloppante vis-à-vis des deux autres B et C. Elle s'étend de a en g. La courbe B a les mêmes valeurs extrêmes enregistrées, mais la courbe c commence seulement en b pour finir en g.

Néanmoins, malgré les différences, ces trois courbes ont bien la même allure, avec le même type de plus grande fréquence placée en d, et avec les mêmes inflexions générales en c et f. Il est vraisemblable que la courbe limite de ces polygones est une courbe binomiale normale n'ayant qu'un sommet principal S. Les deux sommets secondaires s', s'' ne se maintiennent pas quand on dispose de 200 mesures différentes.

Ainsi les graphiques obtenus, dans ce qui va suivre, doivent parfois être interprétés par suite de l'insuffisance relative du nombre des mesures enregistrées.

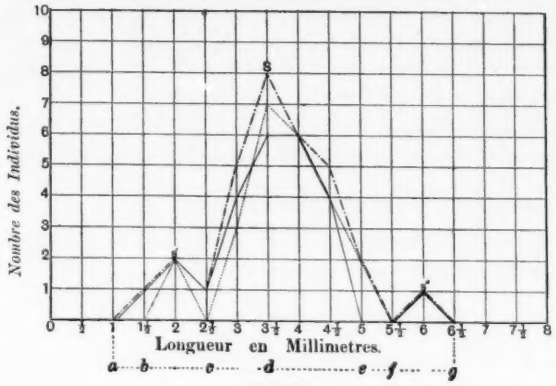


FIG. 2 *a*.

A. —·—·— Courbe tracée avec 31 plantes.
B. ——— Courbe tracée avec 25 plantes.
C. ····· Courbe tracée avec 25 autres plantes.

Laxou. 31—25 plantes isolées de 3 mètres (brévistémonées).
Distance du stigmate à l'anthère.

Même en opérant avec 25 ou 30 échantillons seulement, il y a des polygones de fréquence qui sont souvent très voisins de la courbe limite. L'allure générale du périmètre du polygone renseigne dans ce cas par sa régularité qui n'est évidemment pas l'effet du hasard. Nous l'avons vérifié plusieurs fois pratiquement en comparant deux graphiques obtenus par exemple avec 30 et 140 échantillons différents. Dans le cas des courbes polymorphes il y a parfois une certaine incertitude dans l'interprétation du polygone*, mais dans ce qui va suivre la nature de la courbe est généralement non douteuse, et nous n'avons pas rencontré de courbes de cette dernière catégorie.

Le présent travail apportera donc une solution à la question suivante qui n'a pas encore été abordée je crois :

Quelles sont les diverses courbes des variations de longueur des organes d'une fleur hétérostylée ?

L'hétérostylie est en somme une question de longueurs relatives de deux organes. Le dispositif présente une importance physiologique établie par les recherches sur le croisement†; on peut penser que l'hétérostylie se maintient par suite de conditions particulières dont on peut espérer lire l'existence dans les mesures biométriques.

Avant d'entreprendre ces recherches je pensais, par analogie avec d'autres faits biologiques, qu'un dispositif comme l'hétérostylie ne pouvait sans doute se

* Ludwig, *Bot. Centralblatt*, 1893, 1895. C. B. Davenport, *Statistical Methods.*
† Darwin, *Fécondation croisée. Forme des fleurs.*

maintenir que par suite d'une variation unilatérale. Je supposais que quelques unes des courbes, comme, par exemple, celle qui concerne la distance du stigmate à l'anthère, auraient probablement l'ordonnée maximum à une extrémité de leur étendue, comme dans celles qui sont appelées, à tort par de Vries, demi-courbes Galtoniennes. Comme on le verra par la suite, cette hypothèse était fausse*. Les graphiques sont formels à cet égard.

Pour arriver à apprécier la fixité relative de l'hétérostylie il faut calculer la valeur moyenne de chaque caractère floral, c'est-à-dire établir son type de longueur moyenne, et puis le carré moyen des déviations $= \sqrt{\frac{\Sigma (d^2)}{N}}$.

Pour un nombre N d'échantillons, soit m les mesures en $\frac{1}{2}$ millimètres, et n les nombres exprimant la fréquence de chaque mesure m,

$$\text{La longueur moyenne sera } L = \frac{\Sigma (mn)}{N}.$$

En somme, L est une moyenne arithmétique de toutes les longueurs observées.

D'autre part le type *le plus fréquent* a une longueur K qui est aussi caractéristique des tendances de la race.

Si donc les deux longueurs L et K sont différentes il faut tenir compte des deux pour apprécier la fixité du caractère.

Un exemple graphique fait mieux comprendre cette conclusion : Soient A, B, C trois courbes de fréquence obtenues avec les styles de trois lots de Pulmonaires de trois localités différents :

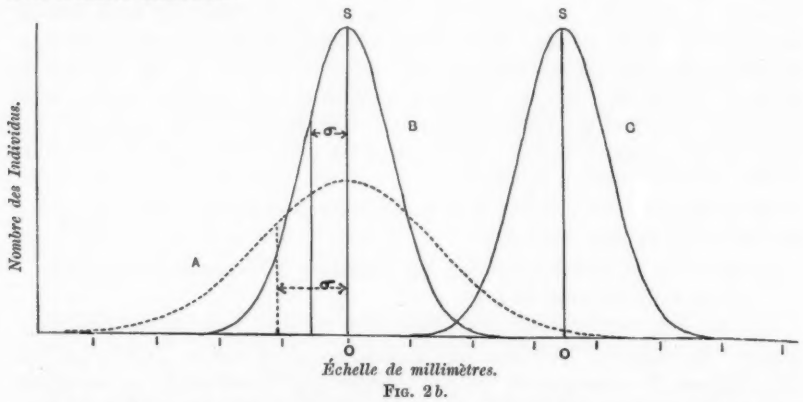


FIG. 2*b*.

Les courbes A et B enferment deux surfaces égales et symétriques par rapport aux axes de grande fréquence qui sont superposés en OS. Elles limitent des polygones de fréquence très différents d'aspect. Le type théorique moyen reste le même.

* Ce n'est pas un cas particulier à la Pulmonaire. Nous publierons ultérieurement une étude biométrique sur la Primevère qui aboutit à cette même conclusion.

La race n'a pas varié de type. Il existe naturellement un nombre indéterminé de courbes de même surface que A et B, et intermédiaires de forme, et auxquelles la conclusion ci-dessus s'applique aussi.

Si la valeur moyenne de la race n'a pas varié, en A et B, il n'est pourtant pas sans intérêt d'enregistrer deux faits :

(1) Dans la station B les individus sont peu variables et leur type de styles est concentré autour d'une longueur moyenne dont ils s'écartent peu ;

(2) Dans la station A les individus ont une longueur des styles qui est très variable et il y a en quelque sorte un affolement du type. Si celui-ci persiste il y a ici une sorte de race géographique différente de l'autre.

Les courbes B et C enferment deux aires égales, et les polygones de fréquence sont identiques et superposables. Le type moyen est très différent en B et C. Il est passé de 5 à $8\frac{1}{2}$, soit une variation, de la taille de la race, qui est une augmentation de plus de 50 p. 100 de sa taille initiale.

Dans une courbe établie à l'aide d'échantillons de localités différentes, il faut donc examiner si, dans les divers lieux, l'ordonnée de plus grande fréquence est déplacée à gauche ou à droite. Dans le travail qui suit, on a cherché à définir le type de longueur moyenne théorique, obtenu à l'aide de mensurations nombreuses dont on prend la moyenne arithmétique. Ce type moyen peut ne pas être le type de plus grande fréquence.

Il ne peut s'en éloigner beaucoup, si la courbe est sensiblement du type des courbes binomiales normales, ou des courbes hyperbinomiales, c'est-à-dire monomorphes, bilatérales et symétriques. Dans le cas d'une courbe parabinomiale, c'est-à-dire dont l'ordonnée de plus grande fréquence est rejetée d'une côté, le type moyen est fixé par l'ordonnée qui divise le polygone de fréquence en deux parties égales, et cette ordonnée n'est jamais la même que celle qui correspond au type le plus fréquent.

Cette dernière est évidemment aussi caractéristique de la race que l'ordonnée du type moyen ; son simple déplacement, sans déformation considérable de la courbe parabinomiale, est une indication certaine de la variation de la race.

Pour construire la courbe qu'on pourra regarder comme représentant la limite théorique du polygone des observations, nous avons adopté la formule symétrique de Gauss, à cause de la symétrie de la plupart des polygones obtenus. Avec le nombre restreint d'échantillons de chaque sorte étudiés, il était inutile d'essayer les formules plus compliquées de Pearson*. Voici donc les quantités que nous avons calculées, pour représenter chaque polygone par la courbe exponentielle de Gauss,

$$y = \frac{N}{\sqrt{\mu\pi}} e^{-\frac{x^2}{\mu}}.$$

* *Phil. Trans.* 1895, et *Biometrika, passim.*

Nous devons d'abord calculer la valeur moyenne du caractère étudié; soit m_1, m_2, etc., les diverses valeurs individuelles dans la série N d'échantillons, nous avons, pour trouver M, la valeur moyenne,

$$M = \frac{\Sigma(m)}{N}.$$

Les valeurs de x dans la formule de Gauss sont les écarts individuels, c'est-à-dire les différences entre les mesures individuelles et la valeur moyenne de la série, ou les valeurs de $(m - M)$. Puisque la valeur la plus grande de y est obtenue en mettant $x = 0$ dans la formule, on voit que la valeur la plus fréquente de la série étudiée doit être la valeur moyenne. Dans les cas où la valeur moyenne ne se trouverait pas plus fréquente qu'aucune autre, on devrait employer l'une ou l'autre des formules asymétriques développées par Pearson (*loc. cit.*).

Pour calculer la quantité μ, on doit calculer la somme des carrés des écarts du moyen, ou la valeur du

$$\Sigma\{(m - M)^2\}:$$

et nous avons

$$\mu = 2\frac{\Sigma\{(m - M)^2\}}{N}.$$

Selon la notation d'Amann (*Journal de Botanique*, T. 13) on pourra écrire

$$\frac{\Sigma\{(m - M)^2\}}{N} = Q^2,$$

et suivant la notation de Pearson on écrira $Q = \sigma$ (index de variabilité).

L'écart probable, la valeur P d'Amann, l'écart quartile de Galton, se trouve facilement par la relation

$$P = 0{\cdot}6745\sigma = 0{\cdot}4769\mu.$$

Dans les tableaux insérés dans ce travail on a donné une importance peut-être exagérée à l'*étendue empirique de la variation*, telle qu'elle résulte souvent d'un très petit nombre d'observations. On a voulu ainsi simplement donner aux botanistes des renseignements concrets sur ce qu'on pouvait s'attendre à rencontrer dans des herborisations analogues aux nôtres. Mais nous n'oublions pas pour cela que cette étendue empirique de la variation *ne doit pas être considérée comme une caractéristique du mode de variation*, parce qu'elle est trop exposée à être modifiée par des causes accidentelles, différentes dans chaque série d'observations. En d'autres termes la loi de variation n'est pas changée parce que les limites enregistrées apparaissent dissemblables dans deux prises successives d'échantillons*.

Il y a lieu au contraire d'attacher de l'importance à l'*étendue théorique de la variation* telle qu'elle se déduit des propriétés caractéristiques de l'exponentielle; et, chaque table de l'intégrale de la Probabilité (celle de Bertrand, par exemple, ou

* Duncker, *Biolog. Centralblatt*, Bd. XVIII. No. 15, S. 569.

celle de Sheppard) nous montre que pour une valeur quelconque de μ ou de σ l'étendue de la variation tend toujours à grandir, quand le nombre des individus étudiés devient plus grand.

Prélèvement des fleurs.

Chaque fleur a été prélevée sur une hampe différente. On sait que chaque hampe florale porte des fleurs du même type morphologique : nous avons plusieurs fois contrôlé ce fait par des mesures précises. Il y a donc une sorte de caractéristique individuelle. Si, par exemple, la fleur bien épanouie, prise comme type, est du type à style exserte par rapport au calice ; on peut constater le même caractère chez les autres fleurs du même pied.

Cette constatation initiale nous a servi de base pour étendre considérablement la portée des statistiques faites. Si les fleurs d'un pied avaient eu des caractéristiques différentes, il eut fallu les mesurer toutes pour en dégager la notion de variabilité maximum. Comme chaque plante possède une moyenne de 13 à 15 fleurs, les mensurations eussent dû être, nécessairement, 13 à 15 fois plus nombreuses pour donner la même approximation que celle que nous avons obtenue.

Le nombre des mesures qui servent de base à ce travail est de près de 5000. Ces mesures ont été fournies par environ un millier de hampes florales.

Prélèvement des échantillons.

On a employé trois méthodes différentes pour choisir les hampes florales qui devaient fournir chacune une fleur à la mensuration.

1ère *méthode.* Elle consiste à explorer une localité de surface limitée (par exemple, 200 à 500 mètres carrés suivant la rareté) et à y prélever toutes les hampes florales sans exception.

Si un rhizôme émet plusieurs hampes, celles-ci doivent présenter une grande ressemblance puisqu'elles proviennent d'une même graine mère. Chaque individu est donc représenté dans la statistique par un nombre de hampes qui est variable.

Cette méthode de prélèvement présente en outre l'inconvénient suivant : Elle suppose que les types brévistylés et les types brévistémonés sont aussi prolifiques au point de vue du nombre des hampes. S'ils ne le sont pas il semble qu'il y ait là une cause qui altère la rigueur de la biométrie comparée des deux types.

2e *méthode.* On prélève comme précédemment, et dans le lot on prend, au hasard, seulement une partie des plantes.

3e *méthode.* On prélève les hampes sur un espace plus grand, en ne ramassant, au hasard, que des hampes espacées de deux ou trois mètres de distance. De cette façon on ne recueille qu'un échantillon de chaque individu issu d'une graine Toutes les hampes récoltées proviennent d'individus et de graines différentes.

Nous avons dressé des graphiques qui permettent de conclure que les trois méthodes peuvent donner des résultats comparables.

La 3e méthode exige pourtant moins d'échantillons différents pour dégager la loi de variation de chaque organe.

Elle est donc supérieure aux autres, et c'est celle que nous avons employée pour la biométrie comparée des Pulmonaires de 4 stations différentes.

Pour distinguer sûrement *P. officinalis* de l'espèce voisine, *P. tuberosa* (var. *latifolia* et var. *angustifolia*), le caractère qui a été choisi a été le suivant: *Carpelles mûrs—aigus au sommet.* La Pulmonaire à racines tubéreuses a, au contraire, des carpelles *arrondis* au sommet. Cette dernière est d'ailleurs beaucoup moins commune que l'autre dans les bois du calcaire jurassique des environs de Nancy.

Plan du travail.

Voici l'ordre des questions abordées:

1. Influence de la taille de la plante sur les dimensions florales. (Plantes récoltées à Maxéville en 1902.)

2. Comparaisons biométriques entre les fleurs brévistémonées et les fleurs brévistylées d'une même localité. (Maxéville, 1903, et Messein, 1903.)

3. Étude comparée des plantes récoltées à plusieurs reprises dans une localité, en 1902 et 1903 (Maxéville), en vue de voir si le climat annuel exerce une influence.

4. Étude comparée des plantes de quatre stations, récoltées la même année dans des localités très différentes d'exposition. (Maxéville, Messein, Laxou, Malzéville.)

5. Conclusions.

I.

Influence de la taille de la plante sur la variation de la fleur.

Le calice de la Pulmonaire officinale est, comme on le sait, gamosépale, et d'un aspect très comparable dans les deux types brévistylé et brévistémoné. Il est renflé et pourvu de cinq lobes dont la hauteur est à peu près de $\frac{1}{4}$ ou $\frac{1}{3}$ de la hauteur totale du calice. Les mensurations ont porté sur la hauteur totale du calice mesuré dans son axe de symétrie.

Les lobes du calice sont de dimensions assez variables pour des calices de même hauteur totale. Voici les deux types extrêmes: Deux calices de 14 millimètres de hauteur totale peuvent être fendus de 6 ou de 3 millimètres seulement. Les dimensions extrêmes de hauteur du calice sont de 8 et 17 millimètres. Le calice, notamment, plus que les autres parties de la fleur, semble devoir varier un peu suivant la vigueur des individus, comme le font les feuilles.

Il est donc nécessaire de rechercher si, aux variations de taille et de vigueur des tiges, correspondent des variations de dimensions pour les diverses parties de la fleur hétérostylée.

Nos mensurations ont porté sur 25 très grandes plantes, et 25 très petites, choisies dans une récolte de plusieurs centaines d'individus brévistémonés d'une localité.

Les grandes tiges avaient 19 à 25 cm. de hauteur.

Les petites tiges atteignaient seulement une hauteur de 8 à 15 cm.

On a prélevé une fleur épanouie et bien développée dans l'inflorescence du sommet de la tige de chaque plante.

Les tables de statistique I et II donnent les mesures enregistrées. Voyez figs. 7—12.

TABLE I.

Mesures de 25 grandes plantes choisies pour leur grande taille (Maxéville, 1902).
(Valeurs en millimètres.)

	1	2	3	4	5		6		7	8	
Nos.	Hauteur des tiges (cm.)	Hauteur du calice (mm.)	Hauteur du stigmate au-dessus de la base du calice	Longueur du style au-dessus de l'ovaire	Distance du stigmate au bord du calice. Style		Hauteur du haut des étamines au-dessus de la base		Distance du stigmate à l'anthère	Nombre de fleurs	
					Inclus dans le calice de	Exserte du calice de	De la corolle	Du calice		Par infl.	Total
1	19	$13\frac{1}{2}$	$12\frac{1}{2}$	$10\frac{1}{2}$	1	—	5	6	$6\frac{1}{2}$	7+7	=14
2	21	$14\frac{1}{2}$	$13\frac{1}{2}$	$11\frac{1}{2}$	1	—	7	8	$5\frac{1}{2}$	9, 6, 5	20
3	20	$13\frac{1}{2}$	$12\frac{1}{2}$	$10\frac{1}{2}$	1	—	6	7	$5\frac{1}{2}$	6, 9	15
4	20	11	12	10	—	1	5	6	6	6, 6	12
5	19	$13\frac{1}{2}$	12	10	$1\frac{1}{2}$	—	5	6	6	7, 8	15
6	22	13	12	10	1	—	7	8	4	6, 10	16
7	20	$12\frac{1}{2}$	12	10	$\frac{1}{2}$	—	$5\frac{1}{2}$	$6\frac{1}{2}$	$5\frac{1}{2}$	7, 6	13
8	20	$12\frac{1}{2}$	$11\frac{1}{2}$	$9\frac{1}{2}$	1	—	6	7	4	7, 12	19
9	19	$13\frac{1}{2}$	$11\frac{1}{2}$	$9\frac{1}{2}$	2	—	6	7	$4\frac{1}{2}$	6, 9	15
10	20	12	11	9	1	—	5	6	5	8, 11	19
11	20	$14\frac{1}{2}$	$13\frac{1}{2}$	$11\frac{1}{2}$	1	—	7	8	$5\frac{1}{2}$	7, 9	16
12	20	12	$11\frac{1}{2}$	$9\frac{1}{2}$	$\frac{1}{2}$	—	$5\frac{1}{2}$	$6\frac{1}{2}$	5	7, 4	11
13	20	13	11	9	2	—	6	7	4	6, 7	13
14	20	$11\frac{1}{2}$	$11\frac{1}{2}$	$9\frac{1}{2}$	—	—	6	7	$4\frac{1}{2}$	6, 9	15
15	25	12	13	11	—	1	$5\frac{1}{2}$	$6\frac{1}{2}$	$6\frac{1}{2}$	6, 7	13
16	20	11	11	9	—	—	$5\frac{1}{2}$	$6\frac{1}{2}$	$4\frac{1}{2}$	5, 9	14
17	19	11	11	9	—	—	5	6	5	8, 5	13
18	19	$12\frac{1}{2}$	12	10	$\frac{1}{2}$	—	5	6	6	6, 8	14
19	23	$11\frac{1}{2}$	12	10	—	$\frac{1}{2}$	7	8	$4\frac{1}{2}$	9, 8	17
20	23	12	12	10	—	—	$5\frac{1}{2}$	$6\frac{1}{2}$	$5\frac{1}{2}$	7, 10	17
21	19	12	$11\frac{1}{2}$	$9\frac{1}{2}$	$\frac{1}{2}$	—	5	6	$5\frac{1}{2}$	6, 15	21
22	23	$14\frac{1}{2}$	12	10	$2\frac{1}{2}$	—	6	7	5	5, 6	11
23	20	12	$12\frac{1}{2}$	$10\frac{1}{2}$	—	$\frac{1}{2}$	$6\frac{1}{2}$	$7\frac{1}{2}$	5	7, 5	12
24	19	12	11	9	1	—	$6\frac{1}{2}$	$7\frac{1}{2}$	$3\frac{1}{2}$	7, 9	16
25	22	12	12	10	—	—	6	7	5	6, 11	17
Variation	19—25	11—$14\frac{1}{2}$	11—$13\frac{1}{2}$	9—$11\frac{1}{2}$	$-2\frac{1}{2}$ à +1		5—7	6—8	$3\frac{1}{2}$—$6\frac{1}{2}$	—	378

TABLE II.

Mesures de 25 petites plantes choisies pour leur petite taille (Maxéville, 1902).
(Valeurs en millimètres.)

	1	2	3	4	5		6		7	8	
Nos.	Hauteur des tiges (cm.)	Hauteur du calice (mm.)	Hauteur du stigmate au-dessus de la base du calice	Longueur du style au-dessus de l'ovaire	Distance du stigmate au bord du calice. Style		Hauteur du haut des étamines au-dessus de la base		Distance du stigmate à l'anthère	Nombre de fleurs	
					Inclus de	Exserte de	De la corolle	Du calice		Par infl.	Total
1	8	12	12	10	—	—	5	6	6	6+7	=13
2	12	16½	13	11	3½	—	6½	7½	5½	6, 9	15
3	13	14	13	11	1	—	6½	7½	5½	7, 10	17
4	13	11	13½	11½	—	2½	7	8	5½	7, 1	8
5	13	12½	12½	10½	—	—	6	7	5½	8, 8	16
6	11	12	11	9	1	—	5	6	5	7, 8	15
7	15	15	14	12	1	—	6½	7½	6½	9, 2	11
8	10	12	12	10	—	—	5	6	6	8, 5	13
9	15	16	13	11	3	—	6	7	6	6, 1	7
10	13	12½	10½	8½	2	—	5½	6½	4	7, 9	16
11	12	13½	12½	10½	1	—	5½	6½	6	6, 9	15
12	11	11½	12	10	—	½	6	7	5	3, 0	3
13	11	13½	12	10	1½	—	6	7	5	6, 0	6
14	14	9½	11½	9½	—	2	6½	7½	4	6, 7	13
15	13	12	12	10	—	—	6	7	5	6, 7	13
16	13	12½	12	10	½	—	6	7	5	8, 11	19
17	14	13	12½	10½	½	—	7	8	4½	7, 7	14
18	14	13	12½	10½	1	—	6½	7½	5	7, 3	10
19	13	14	12	10	2	—	6	7	5	7, 19	26
20	15	13	13	11	—	—	5½	6½	6½	6, 9	15
21	14	10½	11	9	—	½	6	7	4	7, 10	17
22	14	14½	13½	11½	1	—	7	8	6	5, 9	14
23	14	11½	13	11	—	1½	6½	7½	6½	7, 8	15
24	14	12	11	9	1	—	5½	6½	4½	7, 9	16
25	14	12	13	11	—	—	6	7	6	6, 7	13
Variation	8—15	9½—16½	11—14	8½—12	−3½ à +2½		5—7	6—8	4—6½	—	340

Pour comparer avec les plantes brévistémonées voici les mensurations de 2 grandes plantes brévistylées et de 2 petites brévistylées prises au hasard. (Nous n'avons pas cru nécessaire de faire à leur sujet un examen de 50 échantillons.)

TABLE III.

Plantes brévistylées choisies pour leur grande ou leur petite taille (Maxéville, 1902).
(Valeurs en millimètres.)

	Hauteur des tiges (cm.)	Hauteur du calice (mm.)	Hauteur du stigmate au-dessus de la base du calice	Longueur du style au-dessus de l'ovaire	Distance du stigmate au bord du calice	Hauteur du haut des étamines au-dessus de la base		Distance du stigmate à l'anthère	Nombre de fleurs
						De la corolle	Du calice		
Grandes plantes brévistylées	25	13	7	5	6	12	13	5	8+10=18
	22	13	6¾	4¾	6¼	12	13	5¼	8+ 9=17*
Petites plantes brévistylées	12	13	6	4	6	11½	12½	5½	7+6=13
	12	12	7	5	5	12	13	5	7+9=16†

TABLE IV.

Pulmonaires brévistémonées. Variations comparées de 25 grandes et de 25 petites plantes (Maxéville).

Catégories	Grandes plantes hauteur=19—25 cm.	Petites plantes hauteur=8—15 cm.
Proportion centésimale de styles exsertes par rapport au calice	16 p. 100	20 p. 100
Proportion centésimale de styles égaux au calice ...	20 „	24 „
Proportion centésimale de styles inclus dans le calice	64 „	56 „
Hauteur du calice (chiffres extrêmes enregistrés)	11 à 14½ millimètres	9½ à 16½ millimètres
Hauteur du stigmate au-dessus de la base du calice	11 à 13½ „	11 à 14 „
Longueur du style au-dessus de l'ovaire ...	9 à 11½ „	8½ à 11½ „
Distance du stigmate au bord du calice ...	−2½ à +1 „	−3½ à +2½ „
Hauteur du haut des étamines au-dessus de la base du calice	6 à 8 „	6 à 8 „
Distance du stigmate à l'anthère	3½ à 6½ „	4 à 6½ „
Nombre de fleurs par inflorescences ...	4 à 15 fleurs	1 à 19 fleurs
Nombre de fleurs par individu	11 à 21 „	3 à 19 „
Nombre de fleurs pour 25 individus ...	378 „	340 „
Nombre moyen de fleurs par individu ...	15, 12	13, 60

* Soit 17·5 fleurs par plante.

† Soit 14·5 fleurs par plante.

TABLE V.

P. officinalis (*Maxéville* 1902).—*Plantes brévistémonées.* Fig. 8.

Hauteur totale du calice.—Nombre de plantes présentant les diverses hauteurs (pour 25 observations de chaque série).

	9	9½	10	10½	11	11½	12	12½	13	13½	14	14½	15	15½	16	16½	17	Millimètres
Grandes plantes (Table I, col. 2)	—	—	—	—	3	2	8	3	2	4	—	3	—	—	—	—	—	Nombre de plantes
Petites plantes (Table II, col. 2)	—	1	—	1	1	2	6	3	3	2	2	1	1	—	1	1	—	,,

Hauteur du style au-dessus de l'ovaire.—Fréquence relative pour 25 observations. Fig. 10.

	8	8½	9	9½	10	10½	11	11½	12	12½	13	Millimètres
Grandes plantes (Table I, col. 4)	—	—	4	6	9	3	1	2	—	—	—	Nombre de plantes
Petites plantes (Table II, col. 4)	—	1	3	1	7	4	6	2	1	—	—	,,

Distance du stigmate au bord du calice.—Fréquence relative pour 25 observations. Fig. 9 *bis*.

	+3	+2½	+2	+1½	+1	+½	0	−½	−1	−1½	−2	−2½	−3	−3½	−4	Millimètres
Grandes plantes (Table I, col. 5)	—	—	—	—	2	2	5	4	8	1	2	1	--	—	—	Nombre de plantes
Petites plantes (Table II, col. 5)	—	1	1	1	—	2	6	2	7	1	2	—	1	1	—	,,

Hauteur des étamines au-dessus de la base de la corolle.—Fréquence relative. Fig. 11.

	4	4½	5	5½	6	6½	7	7½	Millimètres
Grandes plantes (Table I, col. 6)	—	—	7	5	7	2	4	—	Nombre de plantes
Petites plantes (Table II, col. 6)	—	—	3	4	9	6	3	—	,,

Distance du stigmate à l'anthère.—Fréquence relative. Fig. 9.

	3	3½	4	4½	5	5½	6	6½	7	7½	Millimètres
Grandes plantes (Table I, col. 7)	—	1	3	4	6	6	3	2	—	—	Nombre de plantes
Petites plantes (Table II, col. 7)	—	—	3	2	7	5	6	2	—	—	,,

Nombre de fleurs par inflorescences.—Fréquence relative, pour 25 observations. Fig. 12.

	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	
25 Grandes plantes (Table I, col. 8)	—	—	—	—	1	5	14	12	5	8	2	2	1	—	—	1	—	—	—	—	—	—	—	51 inflorescences
25 Petites plantes (Table II, col. 8)	—	2	1	2	—	2	9	15	6	7	2	1	—	—	—	—	—	—	—	1	—	—	—	48 inflorescences

Nombre de fleurs par individus.—Fréquence relative pour 25 observations. Fig. 7.

	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
Grandes plantes (Table I, col. 8)	–	–	–	–	–	–	–	–	–	2	2	4	3	4	3	3	–	2	1	1	–	–	–	–	–	–	–	–	–
Petites plantes (Table II, col. 8)	–	1	–	–	1	1	1	–	1	1	–	5	2	5	3	2	–	1	–	–	–	–	–	–	1	–	–	–	–

Voici les conclusions ou les observations qu'on peut retirer de la comparaison entre les dimensions des fleurs des 25 grandes plantes et celles des 25 petites plantes.

1. Pour les dimensions du style et du calice, chez les grandes plantes le type semble plus concentré autour de la valeur moyenne (valeur plus grande de σ chez les petites plantes; voyez la Table VI). Ainsi, pour un même nombre (25) d'échantillons de chaque sorte, c'est chez les petites plantes que nous avons rencontré, dans la localité considérée, les plus grands et les plus petits styles, les plus grands et les plus petits calices. Nous n'avons pas ici d'ailleurs la limite extrême possible de la variabilité, puisque cette limite recule naturellement lorsqu'on considère un nombre sans cesse croissant d'échantillons. Par la suite du travail on verra, en effet, que les limites supérieures de la longueur du style ont été enregistrées de la façon suivante :

Avec	50 observations	(Maxéville, 1903) . . .	16·5 mm.
„	173 „	(Maxéville, 1902) . . .	17 „
„	235 „	(Messein, 1903) . . .	18 „
„	476 „	(Maxéville, 1903) . . .	19 „

La chance de trouver un calice très long est d'autant plus petite que la longueur du calice devient plus grande. La rapidité avec laquelle diminue la chance de trouver un calice d'écart donné s'exprime par la valeur de la quantité σ.

TABLE VI.

Valeurs moyennes des caractères, étudiées dans les plantes de grande taille et dans celles de petite taille. (Valeurs en millimètres.)

Caractère	Grandes plantes			Petites plantes		
	Moyenne	σ	$\frac{\sigma}{\sqrt{N}}$	Moyenne	σ	$\frac{\sigma}{\sqrt{N}}$
Hauteur totale du calice	12·52	1·044	0·209	12·78	1·582	0·316
Hauteur du style au-dessus de l'ovaire ...	9·94	0·683	0·136	10·32	0·858	0·172
Distance du stigmate au bord du calice ...	−0·60	0·927	0·186	−0·52	1·345	0·269
Hauteur des étamines au-dessus de la base de la corolle	5·82	0·691	0·138	6·04	0·623	0·125
Distance du stigmate à l'anthère	5·10	0·707	0·141	5·30	0·683	0·137
Nombre de fleurs par inflorescence ...	7·41	1·981	0·277	7·08	2·730	0·394
Nombre de fleurs par individu	15·12	2·658	0·266	13·60	4·499	0·450

Si l'on se bornait à l'examen de cette Table VI. on pourrait, il est vrai, croire à une petite différence du type chez les grandes et chez les petites plantes.

On voit en effet que la valeur moyenne de longueur du style est de 9·94 mm. pour les grandes et 10·32 pour les petites : soit une différence de 0·38 mm. Mais ces valeurs sont sensiblement égales, et la différence apparaît négligeable, lorsqu'on voit la grande valeur de la quantité $\frac{\sigma}{\sqrt{N}}$ dans chaque série. On remarque ainsi que l'étude d'un nombre d'échantillons beaucoup plus grand rapprocherait vraisemblablement ces valeurs moyennes, calculées avec 25 échantillons seulement de chaque sorte.

Chez les petites plantes on rencontre les types de styles les plus inclus et les plus exsertes. Fig. 9 *bis* et Fig. 3.

Pourtant le type moyen semble être le même.

On trouve en effet pour 25 observations de chaque type :

	Grandes plantes	Petites plantes
Styles plus longs que le calice	4	5
Styles égaux au calice	5	6
Styles plus petits que le calice	16	14

Mais il en résulte que les petites plantes ont un style qui a, par rapport au calice, une capacité de croissance plus grande que chez les grandes plantes.

Dans les grandes comme dans les petites plantes il y a donc environ $\frac{3}{5}$ d'incluses, $\frac{1}{5}$ d'exsertes, et $\frac{1}{5}$ de types intermédiaires.

2. La distance du stigmate à l'anthère semble rester très constante comme on le voit, aussi bien au point de vue des valeurs extrêmes que de la longueur moyenne. Fig. 9 et Fig. 3.

Le type le plus fréquent est le type $5\frac{1}{4}$.

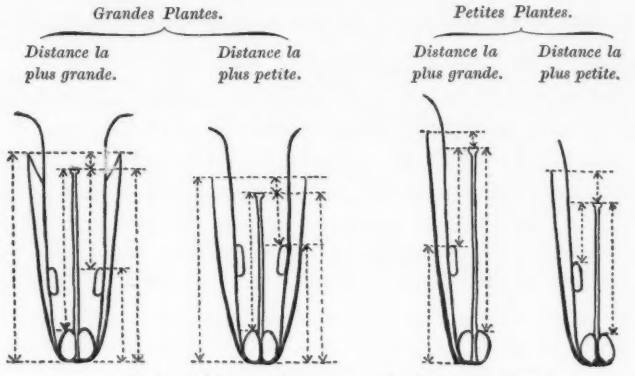
L'amplitude des variations constatées est de 4 à $6\frac{1}{2}$, soit 2·5 d'écart, c'est-à-dire de 1·25 au-dessus et au-dessous du type le plus fréquent.

La courbe ici semble être nettement symétrique.

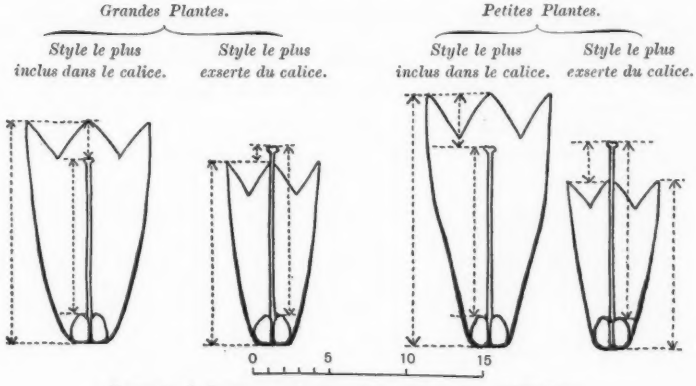
3. Les grandes plantes ont plus de fleurs que les petites :

25 grandes en ont 378, soit 15·1 par plante,
25 petites en ont 340, soit 13·6 par plante.
Différence 38, environ $\frac{1}{9}$ à $\frac{1}{10}$ en plus.

4. On voit de même que chez les petites plantes le nombre de fleurs par individus et par inflorescences est moins constant. C'est chez les petites plantes qu'on trouve les individus et les inflorescences à fleurs les plus nombreuses et les moins nombreuses. Il y a 6 à 7 fleurs par inflorescences et 13 à 15 par individus en général. Fig. 7.



Types extrêmes de distance entre l'anthère et le stigmate.



Rapports de longueur entre le calice et le style. Millimètres.

FIG. 3. Types extrêmes des Tables I. et II.

En résumé, malgré les variations individuelles de taille qu'il est facile de rencontrer, *les types morphologiques des fleurs ne sont pas sensiblement modifiés par la plus ou moins grande vigueur de la végétation.* Nous pouvons constater des différences dans le nombre des fleurs plutôt que des différences dans les dimensions des fleurs et dans les dimensions relatives des parties.

II.

Comparaison entre les plantes brévistylées et les plantes brévistémonées d'une localité. (Maxéville, 1903, et Messein, 1903.)

A Maxéville nous avons fait une récolte de 120 pieds de chaque sorte et nous avons pris au hasard, dans les deux lots, 25 échantillons de plantes dolichostylées et 25 échantillons de plantes brachystylées.

A Messein le nombre des plantes récoltées en une seule fois a été de 140 plantes brévistémonées et 95 plantes brévistylées. Les mensurations qui ont été plus nombreuses qu'à Maxéville (235 plantes au lieu de 50) sont relatives à trois caractères seulement. Elles avaient pour but de vérifier quelques unes des conclusions qu'on pouvait tirer à Maxéville d'un nombre de plantes plus restreint.

Indépendamment d'ailleurs des résultats mentionnés aux Tables Nos. VII et IX, toutes les tables suivantes donnent aussi un grand nombre d'indications sur la même question de comparaison.

On trouvera ci-après :

(1) La Table No. VII des mensurations, et le résumé des variations.

(2) La Table No. VIII des fréquences, tirées de la Table No. VII.

(3) Les graphiques exprimant les résultats de la Table No. VIII. Voyez Figs. 7, 12, et 13 à 18, p. 454.

(4) La Table No. IX et les graphiques qui s'y rapportent (Messein). Voyez Figs. 19—22, p. 455.

(5) Les variations comparées des fleurs brévistylées et brévistémonées.

(6) Des documents sur la fréquence relative des deux types et sur leur floraison comparée, avec statistiques et graphiques.

TABLE VII.

Comparaison entre les Plantes brévistylées et les Plantes brévistémonées d'une localité (Maxéville). (Année 1903.)

25 plantes de chaque sorte prises au hasard dans une récolte de 120 pieds. (Valeurs en millimètres.)

	Brévistémonées								Brévistylées							
Nos. des plantes	Hauteur du calice	Hauteur du stigmate au-dessus de la base du calice	Longueur du style au-dessus de l'ovaire	Distance du stigmate au bord du calice. Style		Distance du haut des étamines au-dessus de la base		Distance du haut du stigmate au sommet de l'anthère	Hauteur du calice	Hauteur du stigmate au-dessus de la base du calice	Longueur du style au-dessus de l'ovaire	Distance du stigmate au bord du calice	Distance de la base des anthères au-dessus de la base de		Distance du stigmate à la base de l'anthère	Distance du haut de l'anthère à la base du calice
				Inclus dans le calice	Exserte du calice	De la corolle	Du calice						La corolle	Du calice		
1	14	13	11	−1	—	7	8	5	14	7	5	7	10½	11½	4½	13½
2	13	13	11	—	—	7	8	5	12½	8	6	4½	10	11	3	13
3	14½	12	10	−2½	—	6½	7½	4½	14½	7	5	7½	10	11	4	13
4	16½	13½	11½	−3	—	7½	8½	5	12½	8	6	4½	9½	10½	2½	12½
5	13½	12	10	−1½	—	6½	7½	4½	14	7½	5½	6½	11	12	4½	14
6	14	12	10	−2	—	6	7	5	10½	7½	5½	3	7½	8½	1	10½
7	14	11½	9½	−2½	—	6½	7½	4	12	6	4	6	11	12	6	14
8	13½	11½	9½	−2	—	7	8	3½	11	7	5	4	10	11	4	13
9	12½	12	10	−½	—	7	8	4	13	6½	4½	6½	10	11	4½	13
10	12	13	11	—	+1	7	8	5	13½	7	5	6½	9	10	3	13
11	15	13½	11½	−1½	—	8	9	4½	14	7	5	7	10	11	4	13
12	12	12½	10½	—	+½	6½	7½	5	12½	8	6	4½	10½	11½	3½	13½
13	15½	15	13	−½	—	7	8	7	12½	7	5	5½	10	11	4	13
14	16½	14	12	−2½	—	7½	8½	5½	16	8	6	8	9½	10½	2½	12½
15	16	14½	12½	−1½	—	7½	8½	6	13	7	5	6	8½	9½	2½	11½
16	12½	11	9	−1½	—	7	8	3	11	7	5	4	7½	8½	1½	10½
17	14	12	10	−2	—	7½	8½	3½	13½	9½	7½	4	10	11	1½	13
18	14	14	12	−1	—	7	8	6	13	7	5	6	10	11	4	13
19	12½	11	9	—	—	6½	7½	3½	16	7	5	9	9½	10½	3½	12½
20	13	13½	11½	−1½	—	6½	7½	6	14½	7½	5½	7	8	9	1½	11
21	13½	12	10	−½	—	7	8	4	14	7½	5½	6½	8½	9½	2	11½
22	13	13½	11½	−1½	—	6½	7½	6	14	9	7	5	11	12	3	14
23	15	13½	11½	−½	—	7	8	5½	14	6½	4½	7½	8½	9½	3	11½
24	15	13½	11½	−1½	—	7½	8½	5	13	7	5	6	9	10	3	12
25	16	13½	11½	−2½	—	8	9	4½	14	7½	5½	6½	10	11	3½	13
Limites de la variation enregistrées avec 25 échantillons	12—16½	11—15	9—13	(−3) (+1)		6—8	7—9	3—7	10½—16	6—9½	4—7½	3—9	7½—11	8½—12	1—6	10½—14

TABLE VIII.

Résumant les fréquences de dimensions obtenues avec les plantes du Table VII.
Figs. 13—18.

Longueur en millimètres	Longueur du style		Distance du stigmate au bord du calice		Distance entre le haut de l'anthère et le haut du calice		Distance du stigmate à l'anthère		Hauteur des étamines au-dessus de la base de la corolle		Hauteur du calice	
	Brévistylées	Brévistémonées	Brévistylées	Brévistémonées	Brévistylées	Brévistémonées	Brévistylées	Brévistémonées	Brévistylées	Brévistémonées	Brévistylées	Brévistémonées
+3	—	—	—	—	—	—	—	—	—	—	—	—
+2½	—	—	—	—	—	—	—	—	—	—	—	—
+2	—	—	—	—	2	—	—	—	—	—	—	—
+1½	—	—	—	—	1	—	—	—	—	—	—	—
+1	—	—	—	1	1	—	—	—	—	—	—	—
+½	—	—	—	1	2	—	—	—	—	—	—	—
0	—	—	—	2	6	—	—	—	—	—	—	—
½	—	—	—	4	3	—	—	—	—	—	—	—
1	—	—	—	1	3	—	1	—	—	—	—	—
1½	—	—	—	8	2	—	3	—	—	—	—	—
2	—	—	—	3	—	—	1	—	—	—	—	—
2½	—	—	—	4	2	—	3	—	—	—	—	—
3	—	—	1	1	—	2	5	1	—	—	—	—
3½	—	—	—	—	3	1	3	3	—	—	—	—
4	1	—	3	—	—	1	5	3	—	—	—	—
4½	2	—	3	—	—	5	3	4	—	—	—	—
5	11	—	1	—	—	6	—	7	—	—	—	—
5½	5	—	1	—	—	2	—	2	—	—	—	—
6	4	—	4	—	—	3	1	4	—	1	—	—
6½	—	—	5	—	—	2	—	—	—	7	—	—
7	1	—	3	—	—	1	—	1	—	10	—	—
7½	1	—	2	—	—	1	—	—	—	5	—	—
8	—	—	1	—	—	—	—	—	—	2	—	—
8½	—	—	—	—	—	1	—	—	—	—	—	—
9	—	2	1	—	—	—	—	—	—	—	—	—
9½	—	2	—	—	—	—	—	—	2	—	—	—
10	—	6	—	—	—	—	—	—	1	—	—	—
10½	—	1	—	—	—	—	—	—	3	—	1	—
11	—	3	—	—	—	—	—	—	2	—	2	—
11½	—	7	—	—	—	—	—	—	3	—	—	—
12	—	2	—	—	—	—	—	—	9	—	1	2
12½	—	1	—	—	—	—	—	—	2	—	4	3
13	—	1	—	—	—	—	—	—	3	—	4	4
13½	—	—	—	—	—	—	—	—	—	—	2	3
14	—	—	—	—	—	—	—	—	—	—	7	4
14½	—	—	—	—	—	—	—	—	—	—	2	1
15	—	—	—	—	—	—	—	—	—	—	—	3
15½	—	—	—	—	—	—	—	—	—	—	—	1
16	—	—	—	—	—	—	—	—	—	—	2	2
16½	—	—	—	—	—	—	—	—	—	—	—	2

TABLE IX.

P. officinalis.—Messein, 1903. Figs. 19—23.

(Pour abréger nous ne donnons pas ici le tableau des *mensurations* détaillées, mais seulement le *tableau des fréquences* qui résume ces mensurations au nombre de plus de 900.)

Hauteur totale du calice.

Dimensions en mm.	9	9½	10	10½	11	11½	12	12½	13	13½	14	14½	15	15½	16	16½	17	17½	18	18½	
Brévistémonées ...	–	–	1	1	1	7	9	9	21	17	21	12	16	6	13	3	1	1	1	–	140 pl.
Brévistylées ...	–	–	1	1	2	4	8	7	10	12	12	12	17	4	3	2	–	–	–	–	95 pl.

Longueur du style.

	3	3½	4	4½	5	5½	6	6½	7	7½	8	8½	9	9½	10	10½	11	11½	12	12½	13	13½	14	14½	15	15½	16	
Brévistémonées ...	–	–	–	–	–	–	–	1	–	–	7	3	11	15	36	20	22	11	9	–	5	–	–	–	–	–	–	140 pl.
Brévistylées ...	–	1	6	16	41	26	5	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	95 pl.

Distance du stigmate à l'anthère.

	0	½	1	1½	2	2½	3	3½	4	4½	5	5½	6	6½	7	7½	8	8½	9	9½	10	
Brévistémonées ...	—	—	1	—	1	1	8	8	18	20	15	22	13	7	2	1	1	1	—	—	—	120 pl.
Brévistylées ...	—	—	—	—	3	10	14	20	17	7	3	—	—	—	—	—	—	—	—	—	—	74 pl.

Distance du stigmate au bord du calice.

	+4	3½	3	2½	2	1½	1	½	0	−½	1	1½	2	2½	3	3½	4	4½	5	5½	6	6½	7	7½	8	8½	9	9½	10	
Brévistémonées ...	—	1	—	—	1	1	3	7	11	19	22	18	23	14	11	6	9	2	—	1	—	1	—	—	—	—	—	—	—	150 pl.
Brévistylées ...	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	4	8	5	16	15	12	19	9	3	1	2	—	95 pl.

TABLE X.

Variations comparées des plantes brévistylées et brévistemonées (Maxéville et Messein).

Catégories	Maxéville 1903 (Résumé du Tableau VIII)		Messein 1903 (Résumé du Tableau IX)	
	Brévistylées 25 plantes	Brévistémonées 25 plantes	Brévistylées 95 plantes	Brévistémonées 120 à 150 plantes
Proportion centésimale de	anthères	styles	anthères	styles
styles ou anthères exsertes du calice ...	24 p. 100	8 p. 100	—	9 p. 100
styles ou anthères égaux au calice	24 „	8 „	—	8 „
styles ou anthères inclus dans le calice ...	52 „	84 „	—	83 „
Hauteur du calice* (chiffres extrêmes) enregistrés avec 25 échantillons ...	millimètres $10\frac{1}{2}$ à 16	millimètres 12 à $16\frac{1}{2}$	millimètres 10 à $16\frac{1}{2}$	millimètres 10 à 18
Hauteur du stigmate au-dessus de la base du calice	$6—9\frac{1}{2}$	11—15	$5\frac{1}{2}—8$	$8\frac{1}{2}—15$
Longueur du style au-dessus de l'ovaire	$4—7\frac{1}{2}$	9—13	$3\frac{1}{2}—6$	$6\frac{1}{2}—13$
Hauteur du pistil au-dessus de la base du calice	$6—9\frac{1}{2}$	11—15	—	—
Distance du stigmate au bord du calice	3—9	+1 à −3	$3\frac{1}{2}—9\frac{1}{2}$	$+3\frac{1}{2}$ à $-6\frac{1}{2}$
Hauteur du haut des étamines au-dessus de la base du calice	$10\frac{1}{2}—14$	$7\frac{1}{2}—9$	—	—
Distance du stigmate à l'anthère ...	$\frac{1}{2}—6$	$\frac{1}{2}—7$	—	—
Nombre des fleurs par inflorescences ...	3 à 11	1 à 15	2 à 5	1 à $8\frac{1}{2}$
Nombre moyen de fleurs par inflorescences	moyenne 7, 1	moyenne 6, 9		
Nombre de fleurs par individu ...	6 à 21	7 à 24		
Nombre de fleurs pour 1000 individus	13554	13573		
Nombre moyen de fleurs par individu	13, 55	13, 57		

* Nous n'avons pas mesuré les dimensions des corolles. Elles sont d'ailleurs aussi très variables quant à la hauteur du tube de la corolle et quant aux dimensions des lames des pétales.

TABLE XI.

Comparaison entre les plantes de Maxéville (Table VIII.) *et celles de Messein* 1903 (Table IX.).

Caractère	Brévistémonées				Brévistylées			
	Maxéville		Messein		Maxéville		Messein	
	Moyenne	σ	Moyenne	σ	Moyenne	σ	Moyenne	σ
Longueur du style	10·82	1·057	10·29	1·142	5·36	0·742	5·03	0·499
Distance du stigmate à l'anthère ...	4·82	1·028	4·85	1·195	3·20	1·394	3·48	1·437
Distance du stigmate au bord du calice	1·30	0·707	1·58	1·472	5·94	1·424	6·68	1·158
Hauteur du calice	14·00	1·349	13·90	1·465	13·30	1·377	13·73	1·370
Hauteur des étamines au-dessus de la corolle	7·00	0·490	—	—	11·56	0·993	—	—
Distance entre le haut de l'anthère et le haut du calice	−5·23	1·314	—	—	−0·62	1·545	—	—

Chez les plantes brévistémonées il y a une ressemblance remarquable du type, dans les deux localités de Messein et Maxéville.

Messein		Maxéville	
83 par 100		84 par 100	de styles inclus
8	„	8	„ de styles affleurant au bord du calice
9	„	8	„ de styles exsertes.

S'on représente par l la longueur du style (Fig. 4), et par d la distance du stigmate au bord du calice, il existe trois types de plantes qui réalisent les rapports de longueur suivants : $l > d$, $l = d$, $l < d$.

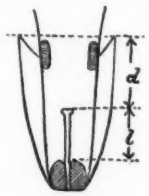


FIG. 4.

Chez les plantes brévistylées de Maxéville et de Messein les différences sont plus grandes :

Maxéville.			Messein.		
68 p. 100 de styles		$l < d$.	80 p. 100 de styles		$l < d$.
0	„	$l = d$.	8	„	$l = d$.
32	„	$l > d$.	12	„	$l > d$.

En ce qui concerne la situation des anthères voici ce qu'on observe à Maxéville :

24 p. 100 d'anthères dépassent le calice. Calice à croissance limitée (très fréquent) ou corolle à croissance exagérée (plus rare).
24 „ „ effleurent le haut du calice.
52 „ „ sont incluses dans le calice.

Pour compléter la comparaison des plantes brévistylées et des plantes brévistémonées voici quelques chiffres : (*a*) sur leur fréquence relative, (*b*) sur leur floraison dans deux autres localités présentant une différence dans la nature du sol.

a. Fréquence du type (p. 1000 plantes brévistylées) :

	A Malzéville sur calcaire jurassique (moyenne 4 prises d'échantillons)	A Essey sur le Lias (1 prise d'échantillon)
Plantes brévistylées	1000	1000
Plantes brévistémonées	936	767

Les plantes brévistylées paraissent donc plus nombreuses d'environ $\frac{1}{10}$ à $\frac{1}{5}$. A Nancy il en serait de même pour *Primula grandiflora*. (D'après une de nos statistiques inédites.) Mais ce calcul présente une certaine incertitude puisqu'il est basé sur les observations d'une seule année.

b. Floraison des Pulmonaires.

Nombre des grappes florales par plante (par 100 plantes observées).

Catégories de plantes	1 grappe	2 grappes	3 grappes	Nombre total de grappes
Brévistémonées ...	3	92	5	202
Brévistylées ...	18	77	5	187

Nombre de fleurs par grappes des divers ordres (par 1000 plantes).

Chiffres calculés avec 40 à 50 plantes seulement de chaque type.

Inflorescences ayant un nombre de fleurs de	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	Totaux	
																	Inflorescence par 1000 plantes	Fleurs pour 1000 plantes
Nombre de grappes des plantes brévistémonées	—	—	55	138	193	416	610	138	249	57	110	—	—	—	—	—	1966	13573
Nombre des grappes brévistylées ...	91	22	—	91	182	320	523	136	182	91	136	68	22	—	22	—	1886	13554

Nombre de fleurs de la grappe terminale (par 1000 plantes).

Nombre des fleurs	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	Nombre moyen par grappe terminale
Nombre des plantes brévistémonées	—	—	55	361	388	55	55	53	33	—	—	—	6·98
Nombre des plantes brévistylées ...	—	22	68	204	408	44	113	47	28	44	22	—	7·46

Les trois tableaux précédents montrent bien qu'il ne semble pas y avoir entre les deux types d'autres particularités que celle-ci :

Les plantes brévistémonées donnent un plus grand nombre d'inflorescences que les brévistylées et le nombre des fleurs n'est pas très différent dans les deux cas.

Mais cette conclusion est-elle générale ? Nous avons cru devoir faire une autre statistique relative au nombre des fleurs afin de voir si la conclusion différente obtenue par Hildebrand ne se trouvait jamais justifiée.

Voici la table de fréquence et le graphique, Fig. 5, relatif à une prise de 33 pieds, isolés de 3 mètres, récoltés à Maxéville en 1903. (Plantes du 4e lot, Table XII.)

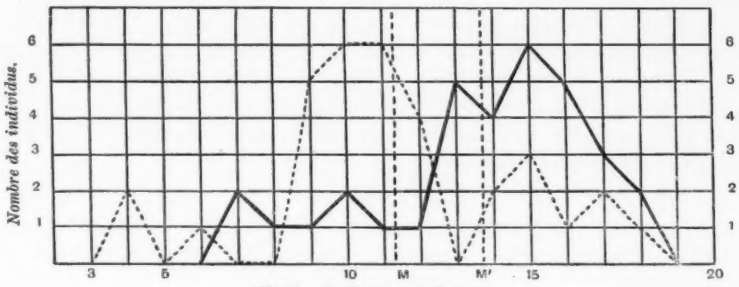


FIG. 5. Nombre de fleurs par plante.

Brévistylées ——— M = Nombre moyen pour les Brévistémonées.
Brévistémonées - - - - - M' = ,, ,, ,, Brévistylées.

Nombre des fleurs par individu.

Nombre des fleurs	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*	*19*	Nombre total	Nombre moyen
Nombre des individus																				
33 plantes brévistémonées	–	–	2	–	1	–	–	5	6	6	4	–	2	3	1	2	1	–	374	11·3
,, brévistylées ...	–	–	–	–	–	2	1	1	2	1	1	5	4	6	5	3	2	–	452	13·7

Ce résultat pourrait sembler confirmer celui qu'a obtenu Hildebrand avec 10 plantes seulement de chaque sorte. Il y a environ 20 °/₀ de fleurs en plus chez les Brévistylées récoltées dans cette localité des environs de Nancy. (Hildebrand avait trouvé 29 °/₀ pour celles qu'il avait observées en Allemagne.) D'autre part, dans une localité voisine de celle qui nous venons d'étudier, à Maxéville même, nous avions trouvé un résultat très différent; 13·5 *fleurs par plante, pour chacun des deux types brévistylés et brévistémonés.* (Voyez Fig. 7, et page 422 : 13554 fleurs brévistylées et 13573 fleurs brévistémonées pour 1000 plantes de chaque sorte.) Nous pensons donc qu'il n'y a pas lieu d'admettre, comme une règle, que les plantes brévistylées sont plus riches en fleurs que les plantes brévistémonées. Il est comme on le voit possible de trouver des localités où il en est tout autrement. D'ailleurs nous avons démontré plus haut que *le nombre des fleurs dépend*

en partie de la vigueur des plantes, puisque chez les plantes brévistémonées de grande taille le nombre moyen de fleurs par pied peut passer de 13·6 à 15·12, et même plus, lorsqu'on s'adresse successivement à des plantes de taille de plus en plus grande. Définitivement nous concluons donc tout autrement que ne l'a fait Hildebrand. Cet auteur ne disposait que d'un trop petit nombre d'échantillons, et ses conclusions ne pouvaient être que très approximatives avec 10 plantes seulement de chaque sorte. Il admettait donc, à tort croyons-nous, et avec lui Darwin aussi, que l'hétérostylie donne deux types d'inégale floraison et que les plantes brévistylées produisent plus de fleurs.

Pour nous il n'en est rien : *Les deux types brévistylés et brévistémonés produisent, dans la région de Nancy, un nombre de fleurs variable, en rapport avec les conditions de végétation, mais non dépendant de l'hétérostylie.* Le chiffre moyen est de 13 à 14 fleurs par plante et nous nous demandons comment Hildebrand a pu donner un chiffre moyen de 28·9 pour les Brévistémonées et 38·7 pour les Brévistylées. S'il n'y a pas d'erreur d'impression dans le mémoire, cet auteur aurait étudié un type de Pulmonaire officinale très différent pour la floraison de celui qui se trouve en Lorraine.

III.

Étude comparée des plantes récoltées deux années de suite, à 5 reprises différentes, dans des localités voisines (Maxéville), années 1902 et 1903.

En 1903, les 4 lots comprenaient au total 238 plantes de chaque sorte, ce qui correspond à 1904 mesures différentes.

Nous avons pensé qu'il n'était pas urgent de donner un tableau général des 1904 mesures ; comme nous l'avons fait précédemment pour la récolte de Messein, nous donnons seulement les tableaux de fréquence qui résument ces mesures.

Néanmoins il nous a paru intéressant de donner les graphiques correspondant à chacun des lots, avant de donner les graphiques qui sont tracés avec les chiffres totaux.

En 1902 le lot unique comprend 138 plantes brévistémonées et 35 brévistylées ; leurs mesures ont été données.

On trouvera donc ci-après :

A. Table XII résumant les fréquences pour chacun des 4 lots de 100, 80, 25, 33 plantes ; et donnant les nombres totaux pour l'ensemble des 238 plantes récoltées à Maxéville en 1903.

B. Graphiques des lots 1, 2, 3, 4. Voyez Figs. 23—36 et 13—18.

C. Graphiques exprimant les résultats pour l'ensemble des 4 lots précédents (1903). Voyez Figs. 53—56.

D. Tables XIII et XIV *c*, des mensurations de 138 plantes brévistémonées et des 35 plantes brévistylées récoltées en 1902 dans le même endroit.

E. Tables XIV*a* et XIV*b* de comparaison des plantes de 1902 et 1903.

F. Graphiques donnant la comparaison des plantes de 1902 et 1903. Voyez Figs. 61—63 et Figs. 53—56.

TABLE XII.

Maxéville, 4 prises différentes (1903), *localités voisines.*

Hauteur totale du calice. Figs. 23, 28, 18, 33.

Nombre de millimètres	8	8½	9	9½	10	10½	11	11½	12	12½	13	13½	14	14½	15	15½	16	16½	17	17½	18	18½	19	19½	Plantes
1er lot, Brévistémonées	—	—	—	—	1	3	8	8	18	6	19	7	11	6	7	4	1	1	—	—	—	—	—	—	100
2e „ „	—	—	—	—	—	2	4	6	13	18	8	12	13	3	—	—	1	—	—	—	—	—	—	—	80
3e „ „	—	—	—	—	—	—	—	—	2	3	4	3	4	1	3	1	2	2	—	—	—	—	—	—	25
4e „ „	—	—	—	—	—	1	—	1	3	2	4	4	9	1	4	3	—	—	—	—	—	—	1	—	33*
Nombre total	—	—	—	—	1	6	12	15	36	29	35	26	37	11	14	8	4	3	—	—	—	—	1	—	238

Nombre de millimètres	8	8½	9	9½	10	10½	11	11½	12	12½	13	13½	14	14½	15	15½	16	16½	17	17½	18	18½	19	19½	Plantes
1er lot, Brévistylées ...	—	1	—	1	—	1	9	10	15	4	11	10	20	6	5	—	4	1	1	1	—	—	—	—	100
2e „ „	—	—	—	—	1	5	6	8	11	10	15	7	11	1	3	1	1	—	—	—	—	—	—	—	80
3e „ „	—	—	—	—	—	1	2	—	1	4	4	2	7	2	—	—	2	—	—	—	—	—	—	—	25
4e „ „	—	—	—	—	—	—	—	3	4	5	2	3	5	3	4	—	3	—	1	—	—	—	—	—	33
Nombre total	—	1	—	1	1	7	17	21	31	23	32	22	43	12	12	1	10	1	2	1	—	—	—	—	238

* Plantes isolées de 3 mètres au moins.

Hauteur du style au-dessus de l'ovaire. Figs. 24, 30, 16, 36.

Nombre de millimètres	2½	3	3½	4	4½	5	5½	6	6½	7	7½	8	8½	9	9½	10	10½	11	11½	12	12½	13	13½	14	Plantes
1er lot, Brévistémonées	—	—	—	—	—	—	—	—	—	—	—	3	6	12	18	31	6	9	4	9	2	—	—	—	100
2e „ „	—	—	—	—	—	—	—	—	—	—	—	1	8	11	14	16	14	9	3	3	—	1	—	—	80
3e „ „	—	—	—	—	—	—	—	—	—	—	—	—	—	2	2	6	1	3	7	2	1	1	—	—	25
4e „ „	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	11	5	5	1	5	3	1	—	—	33
Nombre total	—	—	—	—	—	—	—	—	—	—	—	4	14	26	35	64	26	26	15	19	6	3	—	—	238

Nombre de millimètres	2½	3	3½	4	4½	5	5½	6	6½	7	7½	8	8½	9	9½	10	10½	11	11½	12	12½	13	13½	14	Plantes
1er lot, Brévistylées ...	—	—	3	17	20	37	7	14	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	100
2e „ „	—	1	—	12	21	35	8	3	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	80
3e „ „	—	—	—	1	2	11	5	4	—	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	25
4e „ „	—	—	—	—	2	13	8	5	3	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	33
Nombre total	—	1	3	30	45	96	28	26	5	2	2	—	—	—	—	—	—	—	—	—	—	—	—	—	238

TABLE XII.—(*continued*).

Distance du stigmate à l'anthère. Figs. 25, 29, 14, 34.

Nombre de millimètres	½	1	1½	2	2½	3	3½	4	4½	5	5½	6	6½	7	7½	8	8½	9	Plantes
1er lot, Brévistémonées	—	—	—	—	3	3	8	15	20	25	10	8	7	1	—	—	—	—	100
2e " "	—	—	—	1	2	5	9	7	19	17	8	5	3	3	1	—	—	—	80
3e " "	—	—	—	—	—	1	3	3	4	7	2	4	—	1	—	—	—	—	25
4e " "	—	—	—	—	—	—	1	4	8	5	8	—	3	3	1	—	—	—	33
Nombre total	—	—	—	1	5	9	21	29	51	54	28	17	13	8	2	—	—	—	238

Nombre de millimètres	½	1	1½	2	2½	3	3½	4	4½	5	5½	6	6½	7	7½	8	8½	9	Plantes
1er lot, Brévistylées ...	—	1	4	5	9	18	18	20	9	8	5	3	—	—	—	—	—	—	100
2e " "	—	—	—	1	4	8	13	18	17	11	4	3	—	1	—	—	—	—	80
3e " "	—	1	3	1	3	5	3	5	3	—	—	1	—	—	—	—	—	—	25
4e " "	—	—	—	1	—	3	6	9	6	3	3	1	1	—	—	—	—	—	33
Nombre total	—	2	7	8	16	34	40	52	35	22	12	8	1	1	—	—	—	—	238

Distance du stigmate au bord du calice. Figs. 26, 31, 17, 35.

Nombre de millimètres	0	½	1	1½	2	2½	3	3½	4	4½	5	5½	6	6½	7	7½	8	8½	9	9½	10	10½	11	11½	Plantes
1er lot, Brévistylées ...	1	—	—	—	—	—	2	1	4	8	9	13	12	12	13	8	9	3	2	2	—	—	1	—	100
2e " "	—	—	—	—	—	—	1	—	5	6	14	13	17	6	9	4	2	1	2	—	—	—	—	—	80
3e " "	—	—	—	—	—	—	1	—	3	3	1	1	4	5	3	2	1	—	1	—	—	—	—	—	25
4e " "	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	-	—	—
Nombre total	1	—	—	—	—	—	4	1	12	17	24	27	33	23	25	14	12	4	5	2	—	—	1	—	205

Nombre de millimètres	+2½	+2	+1½	+1	+½	0	−½	1	1½	2	2½	3	3½	4	4½	5	Plantes
1er lot, Brévistémonées	—	2	5	4	5	14	12	14	17	15	5	4	2	1	—	—	100
2e " "	—	1	2	5	3	12	15	16	9	14	2	1	—	—	—	—	80
3e " "	—	—	—	1	1	2	4	1	8	3	4	1	—	—	—	—	25
4e " "	—	—	1	2	2	3	5	7	4	6	2	—	—	1	—	—	33
Nombre total	—	3	8	12	11	31	36	38	38	38	13	6	2	2	—	—	238

Maxéville, 1903. *Longueur relative du style par rapport au calice (Brévistémonées). Proportion par cent.*

	De styles inclus	De styles égaux au calice	De styles exsertes
1er lot (lot total 100) ...	70	14	15
2e lot (lot total 80)	71	15	14
3e lot (25 plantes, parmi 120)	84	8	8
4e lot (100 pieds isolés de 3m)	75	10	15
Moyennes des 4 chiffres	75	12	13

Longueur du style par rapport au calice (Brévistylées). Proportion par cent.

	Longueur du style plus grande que la distance au bord du calice $l>d$	Égale à la distance $l=d$	Longueur du style plus petite que la distance au bord du calice $l<d$
1er lot	17 par 100	13 par 100	70 par 100
2e lot	10 „	13 „	76 „
3e lot	32 „	— „	68 „
4e lot (pieds isolés de 3m)	36 „	15 „	49 „
Moyennes des 4 chiffres	24	10	66

D. TABLE XIII.

Maxéville (1902). *Plantes brévistémonées* (138 *plantes*).

Brévistémonées (138 plantes)													Brévistylées (35 plantes)		
Longueur du style	Hauteur totale du calice	Distance du calice au bord du stigmate	L	H	D	L	H	D	L	H	D		Longueur du style	Hauteur totale du calice	Distance du stigmate au bord du calice
11	14	−1	10	15	−3	9	14	−3	11	14½	−1½		5	14	7
11	10	+3	11½	14½	−1	9½	13½	−2	10	13	−1		5	16	9
11	15	−2	10	13	−1	9	11½	−2½	12½	15	−½		5	13	6
11	14	−1	10½	13½	−1	9½	14	−2½	11	12	+1		4½	14	7½
9	11	—	11	15	−2	11½	14½	−1	10½	13	−½		5	12½	5½
9	15	−4	9	12½	−1½	12½	13½	+1	11	12½	+½		5	15	8
9	12	−1	10	13	−1	10½	13	−½	12	16	−2		4	15	9
11	15	−2	10	13	−1	11½	16	−2½	12	15	−1		6	14	6
10	14	−2	10	13	−1	10½	13	−½	10	14	−2		5½	15½	8
12	17	−3	12	16	−2	9	13	−2	11	15	−1		4	14	8
12	14	—	11½	13½	—	11	15	−2	10	13½	−½		5½	13½	6
10	16	−2	11	13	—	12	16	−2	10	13	−1		5	14	7
11½	13½	—	10	14	−2	9	13½	−1½	10	14	−2		5	13	6
10	13	−1	9½	15	−3½	12	16	−2	10½	11½	+1		5	13½	6½
8½	13	−2½	10	13	−1	10	13	−1	9½	13	−1½		5	14	7
11	14	−1	9½	11½	—	10	13	−1	10½	12½	—		5	13	6
9	14	−3	12	17	+3	11	13	—	11	12½	+½		5	13	6
10	13	−1	11½	13½	—	9	13	−2	9½	13½	−2		5	14	7
11	14½	−1½	10½	14	−1½	11½	14½	−1	8½	12	−1½		5	14	7
9½	14	−2½	12½	14½	—	9	14	−3	10½	17	−4½		5	12½	5½
11	13	—	11½	14	−½	11	13½	−½	11	13	−1		6	15½	7½
10	14	−2	9	12½	−1½	10	13	−1	9	13	−2		6	14	6
11	13	—	11	13	—	11	12	+1	9	13	−2		4	12	6
10	11½	+½	11	15	−2	12	13½	+½	9½	13½	−2		5½	13½	6
10½	12	+1	9	17½	−2½	10	14	−2	10	12	—		5	13½	6½
8	14	−4	10	14	−2	10	13½	−1½	10½	13½	−1		4	13½	7½
10	13	−1	11½	14½	−1	11½	12	+½	8½	10½	—		5½	13½	6
12	12	+2	9½	13½	−2	10	13	−1	11½	13	+½		5	14	7
9	12	−1	10	11½	+½	12	16	−2½	8½	10	+½		6	14	6
10	12	—	11½	15½	−2	9	15	−4	8½	12	−1½		5	13	6
10	12½	−1	10	13½	−1½	11	15	−2	8½	15	−4½		4½	15	8½
11	17	−4	11	14	−1	11	13	—	8	12	−2		6	14½	6½
10½	16	−3½	9½	14½	−3	9	15	−4	10	12	—		5	15	8
11	14	−1	10½	13	−½	10	15	−3	10	13	−1		5	13	6
11½	16	−2½	12½	13½	+1								5½	14	6½
			Amplitude de la variation ...						8 à 12½	10 à 17	+3 à −4½		4—6	12—16	5½—9

Chez les plantes brévistémonées il y a

76 p. 100 de styles inclus.
14 „ de styles égaux au calice.
10 „ de styles exsertes.

Nous plaçant au point de vue des renseignements botaniques faciles à vérifier, nous donnons ci-dessous quelques unes des dimensions observées. Elles peuvent être retenues au point de vue de la botanique descriptive et comparative des Pulmonaires de stations différentes.

E. TABLE XIV*a*.

Comparaisons des plantes de 1902, *et des plantes de* 1903, *d'une même localité* (*Maxéville*).

	Maxéville	
	Année 1902	Année 1903
	Brévistémonées (1902)	Brévistémonées (1903)
Proportion centésimale { styles exsertes du calice	10 par 100	13 par 100
„ égaux au calice	14 „	12 „
„ inclus dans le calice	76 „	75 „
Hauteur du calice { chiffres extrêmes enregistrés sur les échantillons observés	10 à 17 mm.	10 à 19 mm.
chiffre de grande fréquence	13 „	13 ou 14 „
Hauteur du stigmate au-dessus de la base du calice	„ „	10 à 15 „
Longueur du style au-dessus de l'ovaire { chiffres extrêmes observés	8—12½ „	8—13 „
chiffre de grande fréquence	10 „	10 „
Distance du stigmate au bord du calice { chiffres extrêmes	+3 à −4½ „	+2 à −4 „
chiffre de grande fréquence	−1 „	−1 à −2 „
Distance du stigmate à l'anthère { chiffres extrêmes observés ...	„ „	2—7½ „
chiffre de grande fréquence ...	„ „	4½ ou 5 „
	Brévistylées (1902)	Brévistylées (1903)
Hauteur du calice { chiffres extrêmes observés	12 à 16 mm.	8½ à 17½ mm.
chiffre de grande fréquence	14 „	14 „
Hauteur du stigmate au-dessus de la base du calice	„ „	5 à 9½ „
Longueur du style au-dessus de l'ovaire { chiffres extrêmes observés	4—6 „	3—7½ „
chiffre de grande fréquence	5 „	5 „
Distance du stigmate au bord du calice { chiffres extrêmes observés	−5½ à −9 „	0 à −11 „
chiffre de grande fréquence	−6 „	−6 „
Distance du stigmate à l'anthère { chiffres extrêmes observés ...	„ „	1 à 7 „
chiffre de grande fréquence ...	„ „	4 „

Les chiffres ci-dessous offrent un intérêt plus considérable au point de vue de l'étude mathématique de la variation :

TABLE XIV*b*.

Maxéville 1902 *et* 1903.

	Brévistémonées				Brévistylées			
	1902		1903		1902		1903	
	Moyen	σ	Moyen	σ	Moyen	σ	Moyen	σ
Hauteur totale du calice	13·62	1·369	13·12	1·377	13·87	0·897	13·04	1·474
Hauteur du style	10·37	1·029	10·21	1·081	5·07	0·549	4·99	0·700
Distance du stigmate à l'anthère ...	—	—	—	—	—	—	3·84	1·070
Distance du stigmate au bord de la calice	−1·26	1·305	−0·92	1·159	6·80	0·920	6·05	1·439

F. *P. officinalis, Maxéville,* 1902. *Table XIV c. obtenu avec les chiffres mentionnés à la Table XIII.*

Hauteur totale du calice. Fig. 63.

Dimensions en mm.	*9*	*9½*	*10*	*10½*	*11*	*11½*	*12*	*12½*	*13*	*13½*	*14*	*14½*	*15*	*15½*	*16*	*16½*	*17*	*17½*	*18*	*18½*	*19*	Plantes
Brévistémonées ...	—	—	2	1	1	5	13	6	32	19	22	7	16	1	9	—	4	—	—	—	—	138
Brévistylées ...	—	—	—	—	—	—	1	2	6	6	12	1	4	2	1	—	—	—	—	—	—	35

Longueur du style. Fig. 62.

Dimensions en mm.	*3*	*3½*	*4*	*4½*	*5*	*5½*	*6*	*6½*	*7*	*7½*	*8*	*8½*	*9*	*9½*	*10*	*10½*	*11*	*11½*	*12*	*12½*	*13*	*13½*	Plantes
Brévistémonées ...	—	—	—	—	—	—	—	—	—	—	2	5	18	10	34	13	28	13	11	4	—	—	138
Brévistylées ...	—	—	4	2	19	5	5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	35

Distance du stigmate au bord du calice. Fig. 61.

Dimensions en mm.	+3½	*3*	*2½*	*2*	*1½*	*1*	+½	*0*	−½	*−1*	*1½*	*2*	*2½*	*3*	*3½*	*4*	*4½*	*5*	*5½*	*6*	*6½*	*7*	*7½*	*8*	*8½*	*9*	*9½*	Plantes
Brévistémonées ...	—	1	—	1	—	5	8	18	10	31	12	28	7	8	2	5	2	—	—	—	—	—	—	—	—	—	—	138
Brévistylées ...	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	13	4	6	3	4	1	2	—	35

Chez les plantes brévistémonées il y a

76 par 100 de styles inclus.
14 „ de styles égaux au calice.
10 „ de styles exsertes.

Conclusions relatives à la fixité du type pendant deux années successives.

On voit en comparant les graphiques (Figs. 53—56 et Figs. 61—63) que les résultats sont très semblables pour les deux années 1902 et 1903:

C'est ainsi que le maximum de fréquence de la longueur des deux styles est sur les chiffres 5 et 10 dans les deux cas, et malgré le nombre faible des observations faites.

La position relative des deux courbes pour la distance du stigmate à l'anthère est la même en 1902 pour tous les lots.

Il en est de même pour la position des 2 maxima de fréquence de la distance du stigmate au bord du calice: ils sont en 1902 et 1903 sur − 1 chez les Brévistémonées et + 6 chez les Brévistylées.

Le maximum des fréquences de la hauteur du calice est aussi dans tous les cas aux environs de 13 à 14.

On voit donc que l'influence du climat de l'année ne semble pas modifier les capacités de croissance des diverses parties de la fleur hétérostylée.

IV.

ÉTUDE COMPARÉE DES PLANTES DE QUATRE STATIONS DIFFÉRENTES DES ENVIRONS DE NANCY.

Nous avons récolté des plantes espacées de 3 mètres environ, afin de ne mesurer que des pieds issus de graines différentes.

Le nombre des plantes mesurées a été de 25 au moins pour chacune des stations de

Laxou, Malzéville, Maxéville, Messein.

Voici la série des documents mentionnés ci-après:

G. Tables XV, XVI, XVII, XVIII, donnant les mensurations des plantes de chacune des quatre localités.

H. Tables XIX, XX, XXI, XXII. Fréquences relatives des diverses longueurs étudiées.

I. Tables XXIII A et XXIII B. Variabilité comparative dans les quatre stations.

J. Schémas représentant les types extrêmes ou caractéristiques de chaque station. Voyez p. 451.

K. Graphiques comparés, relatifs aux Tables XIX, XX, XXI, XXII. Voyez Figs. 37—48, Figs. 49—52, et Figs. 57—60, pp. 457—8.

L. Des documents précédents nous pouvons conclure ensuite sur

Les caractères généraux des races géographiques observées,

Les types moyens théoriques calculés,

La variabilité et la fixité de l'hétérostylie chez la Pulmonaire.

G. Tableaux des mensurations des plantes de quatre localités différentes.

TABLE XV.

Pulmonaires récoltées à Laxou. (*Année* 1903.)

Les pieds récoltés étaient situés à 3 mètres de distance au moins. Valeurs en millimètres.

Brévistylées								Brévistémonées						
Nos. des plantes	Nos. des dessins schématiques	Hauteur de l'étamine	Distance de l'anthère à la base du calice	Distance du haut du stigmate à la base de l'anthère	Longueur du style et du stigmate réunis	Hauteur totale du calice	Distance du stigmate au bord du calice	Nos. des plantes	Nos. des dessins schématiques	Hauteur de l'étamine	Distance du haut du stigmate à la partie supérieure de l'anthère	Longueur du style et du stigmate	Hauteur totale du calice	Distance du stigmate au bord du calice
1	—	12	10	$3\frac{1}{2}$	$4\frac{1}{2}$	15	$8\frac{1}{2}$	*1a*	—	$7\frac{1}{2}$	$2\frac{1}{2}$	8	$13\frac{1}{2}$	$-3\frac{1}{2}$
2	—	11	9	3	4	$11\frac{1}{2}$	$5\frac{1}{2}$	*2*	—	$7\frac{1}{2}$	3	$8\frac{1}{2}$	$14\frac{1}{2}$	-4
3	—	$11\frac{1}{2}$	$9\frac{1}{2}$	3	$4\frac{1}{2}$	15	$8\frac{1}{2}$	*3*	—	7	$4\frac{1}{2}$	$9\frac{1}{2}$	$14\frac{1}{2}$	-3
4	4	$10\frac{1}{2}$	$8\frac{1}{2}$	$2\frac{1}{2}$	4	13	7	*4*	—	$7\frac{1}{2}$	3	$8\frac{1}{2}$	$13\frac{1}{2}$	-3
5	10–14	14	12	5	5	$16\frac{1}{2}$	$9\frac{1}{2}$	*5*	—	7	$3\frac{1}{2}$	$8\frac{1}{2}$	$13\frac{1}{2}$	-3
6	—	12	10	$3\frac{1}{2}$	$4\frac{1}{2}$	$14\frac{1}{2}$	8	*6*	2 bis & 7	8	$1\frac{1}{2}$	$7\frac{1}{2}$	$14\frac{1}{2}$	-5
7	—	$10\frac{1}{2}$	$8\frac{1}{2}$	$2\frac{1}{2}$	4	12	6	*7*	—	7	4	9	$13\frac{1}{2}$	$-2\frac{1}{2}$
8	—	12	10	4	4	12	6	*8*	—	$6\frac{1}{2}$	$3\frac{1}{2}$	8	$14\frac{1}{2}$	$-4\frac{1}{2}$
9	—	$11\frac{1}{2}$	$9\frac{1}{2}$	$3\frac{1}{2}$	4	$12\frac{1}{2}$	$6\frac{1}{2}$	*9*	—	$6\frac{1}{2}$	$4\frac{1}{2}$	9	14	-3
10	—	$11\frac{1}{2}$	$9\frac{1}{2}$	$2\frac{1}{2}$	5	$13\frac{1}{2}$	$6\frac{1}{2}$	*10*	—	6	$3\frac{1}{2}$	$8\frac{1}{2}$	14	$-3\frac{1}{2}$
11	—	12	10	3	5	16	9	*11*	—	$6\frac{1}{2}$	$3\frac{1}{2}$	8	12	-2
12	—	$10\frac{1}{2}$	$8\frac{1}{2}$	$2\frac{1}{2}$	4	13	7	*12*	—	7	4	9	13	-2
13	—	$10\frac{1}{2}$	$8\frac{1}{2}$	$2\frac{1}{2}$	4	13	7	*13*	—	$6\frac{1}{2}$	$4\frac{1}{2}$	9	$13\frac{1}{2}$	$-2\frac{1}{2}$
14	—	$11\frac{1}{2}$	$9\frac{1}{2}$	3	$4\frac{1}{2}$	15	$8\frac{1}{2}$	*14*	2—13	$6\frac{1}{2}$	3	$7\frac{1}{2}$	$10\frac{1}{2}$	-1
15	—	11	9	2	5	14	7	*15*	8	6	6	10	15	-3
16	—	11	9	$2\frac{1}{2}$	$4\frac{1}{2}$	14	$7\frac{1}{2}$	*16*	—	8	2	8	$14\frac{1}{2}$	$-4\frac{1}{2}$
17	9	$11\frac{1}{2}$	$9\frac{1}{2}$	$1\frac{1}{2}$	6	$15\frac{1}{2}$	$7\frac{1}{2}$	*17*	—	8	2	8	$14\frac{1}{2}$	$-4\frac{1}{2}$
18	—	$11\frac{1}{2}$	$9\frac{1}{2}$	$3\frac{1}{2}$	4	14	8	*18*	—	7	4	9	13	-2
19	—	11	9	$2\frac{1}{2}$	$4\frac{1}{2}$	13	$6\frac{1}{2}$	*19*	—	$7\frac{1}{2}$	4	$9\frac{1}{2}$	$14\frac{1}{2}$	-3
20	—	$11\frac{1}{2}$	$9\frac{1}{2}$	3	$4\frac{1}{2}$	13	$6\frac{1}{2}$	*20*	—	7	4	9	14	-3
21	—	11	9	2	5	16	9	*21*	—	7	$3\frac{1}{2}$	$8\frac{1}{2}$	14	$-3\frac{1}{2}$
22	—	11	9	$1\frac{1}{2}$	$5\frac{1}{2}$	13	$5\frac{1}{2}$	*22*	—	$7\frac{1}{2}$	3	$8\frac{1}{2}$	$12\frac{1}{2}$	-2
23	—	11	9	2	5	12	5	*23*	—	7	$4\frac{1}{2}$	$9\frac{1}{2}$	13	$-1\frac{1}{2}$
24	3	13	11	3	6	15	7	*24*	—	8	$3\frac{1}{2}$	$9\frac{1}{2}$	13	$-1\frac{1}{2}$
25	—	$12\frac{1}{2}$	$10\frac{1}{2}$	$3\frac{1}{2}$	5	$14\frac{1}{2}$	$7\frac{1}{2}$	*25*	—	7	4	9	$14\frac{1}{2}$	$-3\frac{1}{2}$
								26	—	$7\frac{1}{2}$	$4\frac{1}{2}$	9	13	-2
								27	1	$7\frac{1}{2}$	5	$10\frac{1}{2}$	$13\frac{1}{2}$	-1
								28	11	$7\frac{1}{2}$	$3\frac{1}{2}$	9	17	-6
								29	—	7	$3\frac{1}{2}$	$8\frac{1}{2}$	13	$-2\frac{1}{2}$
								30	—	$6\frac{1}{2}$	5	$9\frac{1}{2}$	$13\frac{1}{2}$	-2
								31	—	$7\frac{1}{2}$	3	$8\frac{1}{2}$	14	$-3\frac{1}{2}$
Valeurs extrêmes		$10\frac{1}{2}$–14	$8\frac{1}{2}$–12	$1\frac{1}{2}$–5	4–6	$11\frac{1}{2}$–$16\frac{1}{2}$	5–9	Valeurs extrêmes		6–8	$1\frac{1}{2}$–6	$7\frac{1}{2}$–$10\frac{1}{2}$	$10\frac{1}{2}$–17	-6 à -1

TABLE XVI.

Pulmonaires récoltées à Malzéville. (*Année* 1903.)

Les pieds récoltés étaient situés à 3 mètres de distance au moins dans une station regardant le midi.

Longueurs exprimées en millimètres.

Brévistylées								Brévistémonées						
Nos. des plantes	Nos. des dessins schématiques	Hauteur de l'étamine	Distance de l'anthère à la base du calice	Distance du haut du stigmate à la base de l'anthère	Longueur du style et du stigmate réunis	Hauteur totale du calice	Distance du stigmate au bord du calice	Nos. des plantes	Nos. des dessins schématiques	Hauteur de l'étamine	Distance du haut du stigmate à la partie supérieure de l'anthère	Longueur du style et du stigmate	Hauteur totale du calice	Distance du stigmate au bord du calice
1	—	12	10	$3\frac{1}{2}$	$4\frac{1}{2}$	11	$4\frac{1}{2}$	*1*	—	$6\frac{1}{2}$	$4\frac{1}{2}$	9	14	−3
2	—	13	11	4	5	13	6	*2*	—	$6\frac{1}{2}$	$5\frac{1}{2}$	10	12	—
3	19	12	10	2	6	14	6	*3*	—	$7\frac{1}{2}$	4	$9\frac{1}{2}$	13	$-1\frac{1}{2}$
4	—	13	11	4	5	13	6	*4*	—	$7\frac{1}{2}$	5	$10\frac{1}{2}$	14	$-1\frac{1}{2}$
5	—	13	11	4	5	13	6	*5*	—	7	5	10	$12\frac{1}{2}$	$-\frac{1}{2}$
6	—	$11\frac{1}{2}$	$9\frac{1}{2}$	3	$4\frac{1}{2}$	$11\frac{1}{2}$	5	*6*	—	$6\frac{1}{2}$	$3\frac{1}{2}$	8	12	−2
7	—	12	10	$3\frac{1}{2}$	$4\frac{1}{2}$	$11\frac{1}{2}$	5	*7*	17	7	6	11	13	—
8	—	12	10	$3\frac{1}{2}$	$4\frac{1}{2}$	12	$5\frac{1}{2}$	*8*	—	$7\frac{1}{2}$	$5\frac{1}{2}$	11	$14\frac{1}{2}$	$-1\frac{1}{2}$
9	20—30	$12\frac{1}{2}$	$10\frac{1}{2}$	$4\frac{1}{2}$	4	15	9	*9*	—	$7\frac{1}{2}$	$4\frac{1}{2}$	10	14	−2
10	—	13	11	3	6	$11\frac{1}{2}$	$3\frac{1}{2}$	*10*	—	$6\frac{1}{2}$	5	$9\frac{1}{2}$	12	$-\frac{1}{2}$
11	—	11	9	$2\frac{1}{2}$	$4\frac{1}{2}$	8	$1\frac{1}{2}$	*11*	18	6	$3\frac{1}{2}$	$7\frac{1}{2}$	$10\frac{1}{2}$	−1
12	—	$11\frac{1}{2}$	$9\frac{1}{2}$	$3\frac{1}{2}$	4	10	4	*12*	—	$7\frac{1}{2}$	4	$9\frac{1}{2}$	$12\frac{1}{2}$	−1
13	—	$12\frac{1}{2}$	$10\frac{1}{2}$	$3\frac{1}{2}$	5	11	4	*13*	29	$6\frac{1}{2}$	$4\frac{1}{2}$	9	$10\frac{1}{2}$	$+\frac{1}{2}$
14	—	$11\frac{1}{2}$	$9\frac{1}{2}$	2	$5\frac{1}{2}$	13	$6\frac{1}{2}$	*14*	—	$8\frac{1}{2}$	$3\frac{1}{2}$	10	12	—
15	—	$11\frac{1}{2}$	$9\frac{1}{2}$	3	$4\frac{1}{2}$	$11\frac{1}{2}$	5	*15*	—	$7\frac{1}{2}$	$4\frac{1}{2}$	10	13	−1
16	—	12	10	3	5	12	5	*16*	24	6	7	11	$13\frac{1}{2}$	$-\frac{1}{2}$
17	—	12	10	3	5	$12\frac{1}{2}$	$5\frac{1}{2}$	*17*	—	7	$3\frac{1}{2}$	$8\frac{1}{2}$	13	$-2\frac{1}{2}$
18	25	12	10	2	6	$12\frac{1}{2}$	$4\frac{1}{2}$	*18*	—	$6\frac{1}{2}$	5	$9\frac{1}{2}$	$11\frac{1}{2}$	—
19	—	12	10	$3\frac{1}{2}$	$4\frac{1}{2}$	$12\frac{1}{2}$	6	*19*	—	$6\frac{1}{2}$	$3\frac{1}{2}$	8	$11\frac{1}{2}$	$-1\frac{1}{2}$
20	—	11	9	2	5	11	4	*20*	—	8	4	10	12	—
21	—	12	10	3	5	12	5	*21*	27	7	$4\frac{1}{2}$	$9\frac{1}{2}$	$14\frac{1}{2}$	−3
22	—	$11\frac{1}{2}$	$9\frac{1}{2}$	3	$4\frac{1}{2}$	$13\frac{1}{2}$	7	*22*	23	7	3	8	12	−2
23	—	13	11	$4\frac{1}{2}$	$4\frac{1}{2}$	$12\frac{1}{2}$	6	*23*	—	$7\frac{1}{2}$	$4\frac{1}{2}$	10	14	−2
24	—	13	11	5	4	13	7	*24*	—	6	4	8	12	−2
25	—	$12\frac{1}{2}$	$10\frac{1}{2}$	4	$4\frac{1}{2}$	12	$5\frac{1}{2}$	*25*	—	7	6	11	$12\frac{1}{2}$	$+\frac{1}{2}$
Valeurs extrêmes		11–13	9–11	2–5	4–6	8–15	$1\frac{1}{2}$–9	Valeurs extrêmes		6–$8\frac{1}{2}$	3–7	$7\frac{1}{2}$–11	$10\frac{1}{2}$–$14\frac{1}{2}$	−3 à $+\frac{1}{2}$

TABLE XVII.

Pulmonaires récoltées à Maxéville. (*Année* 1903.)

(Les pieds récoltés étaient situés à 3 mètres de distance au moins dans une station regardant le nord.)

Longueurs exprimées en millimètres.

Brévistylées								Brévistémonées						
Nos. des plantes	Nos. des dessins schématiques	Hauteur de l'étamine	Distance de l'anthère à la base du calice	Distance du haut du stigmate à la base de l'anthère	Longueur du style et du stigmate Hauteur du sommet du stigmate au-dessus de l'ovaire	Hauteur totale du calice	Distance du stigmate au bord du calice	Nos. des plantes	Nos. des dessins schématiques	Hauteur de l'étamine	Distance du haut du stigmate à la base de l'anthère supérieure	Longueur du style et du stigmate	Hauteur totale du calice	Distance du stigmate au bord du calice
1	64	11	9	3	4	$9\frac{1}{2}$	$3\frac{1}{2}$	*1*	—	$6\frac{1}{2}$	5	$9\frac{1}{2}$	11	$+\frac{1}{2}$
2	—	$11\frac{1}{2}$	$9\frac{1}{2}$	$2\frac{1}{2}$	5	13	6	*2*	—	7	$4\frac{1}{2}$	$9\frac{1}{2}$	11	$+\frac{1}{2}$
3	—	$10\frac{1}{2}$	$8\frac{1}{2}$	2	$4\frac{1}{2}$	13	$6\frac{1}{2}$	*3*	—	$5\frac{1}{2}$	5	$8\frac{1}{2}$	12	$-1\frac{1}{2}$
4	—	$12\frac{1}{2}$	$10\frac{1}{2}$	$3\frac{1}{2}$	5	$12\frac{1}{2}$	$5\frac{1}{2}$	*4*	50	7	$3\frac{1}{2}$	$8\frac{1}{2}$	14	$-3\frac{1}{2}$
5	—	11	9	$2\frac{1}{2}$	$4\frac{1}{2}$	$11\frac{1}{2}$	5	*5*	—	7	$3\frac{1}{2}$	$8\frac{1}{2}$	12	$-1\frac{1}{2}$
6	—	12	10	3	5	13	6	*6*	49 & 56	$7\frac{1}{2}$	$6\frac{1}{2}$	12	14	—
7	—	12	10	$2\frac{1}{2}$	$5\frac{1}{2}$	12	$4\frac{1}{2}$	*7*	—	7	$3\frac{1}{2}$	$8\frac{1}{2}$	12	$-1\frac{1}{2}$
8	—	$11\frac{1}{2}$	$9\frac{1}{2}$	$2\frac{1}{2}$	5	12	5	*8*	—	6	5	9	$11\frac{1}{2}$	$-\frac{1}{2}$
9	—	$12\frac{1}{2}$	$10\frac{1}{2}$	$4\frac{1}{2}$	4	$12\frac{1}{2}$	$6\frac{1}{2}$	*9*	—	7	$4\frac{1}{2}$	$9\frac{1}{2}$	$12\frac{1}{2}$	-1
10	52	$11\frac{1}{2}$	$9\frac{1}{2}$	$3\frac{1}{2}$	4	13	7	*10*	—	$6\frac{1}{2}$	4	$8\frac{1}{2}$	$12\frac{1}{2}$	-2
11	—	13	11	4	5	14	7	*11*	—	$7\frac{1}{2}$	$4\frac{1}{2}$	9	11	—
12	—	12	10	3	5	13	6	*12*	—	7	$5\frac{1}{2}$	$10\frac{1}{2}$	13	$-\frac{1}{2}$
13	—	11	9	2	5	12	5	*13*	61	$6\frac{1}{2}$	$5\frac{1}{2}$	10	11	$+1$
14	—	11	9	2	5	11	4	*14*	—	8	$4\frac{1}{2}$	$10\frac{1}{2}$	14	$-1\frac{1}{2}$
15	51	$11\frac{1}{2}$	$9\frac{1}{2}$	$1\frac{1}{2}$	6	14	6	*15*	—	8	$5\frac{1}{2}$	$11\frac{1}{2}$	14	$-\frac{1}{2}$
16	—	$11\frac{1}{2}$	$9\frac{1}{2}$	2	$5\frac{1}{2}$	$11\frac{1}{2}$	4	*16*	—	7	$4\frac{1}{2}$	9	13	$-1\frac{1}{2}$
17	57	$10\frac{1}{2}$	$8\frac{1}{2}$	$1\frac{1}{2}$	5	11	4	*17*	—	$7\frac{1}{2}$	$4\frac{1}{2}$	10	$13\frac{1}{2}$	$-1\frac{1}{2}$
18	—	11	9	2	5	$12\frac{1}{2}$	$5\frac{1}{2}$	*18*	55	7	$3\frac{1}{2}$	$8\frac{1}{2}$	14	$-3\frac{1}{2}$
19	—	13	11	4	5	12	5	*19*	—	$6\frac{1}{2}$	$5\frac{1}{2}$	10	12	—
20	62	12	10	2	6	$14\frac{1}{2}$	$6\frac{1}{2}$	*20*	59	7	4	9	15	-4
21	—	14	12	5	5	13	6	*21*	—	7	$5\frac{1}{2}$	$10\frac{1}{2}$	14	$-1\frac{1}{2}$
22	—	$11\frac{1}{2}$	$9\frac{1}{2}$	3	$4\frac{1}{2}$	$11\frac{1}{2}$	5	*22*	—	7	5	10	14	-2
23	58	$11\frac{1}{2}$	$9\frac{1}{2}$	$3\frac{1}{2}$	4	13	7	*23*	—	$7\frac{1}{2}$	$5\frac{1}{2}$	11	13	—
24	—	12	10	$3\frac{1}{2}$	$5\frac{1}{2}$	13	$5\frac{1}{2}$	*24*	—	7	$4\frac{1}{2}$	$9\frac{1}{2}$	$13\frac{1}{2}$	-2
25	—	$10\frac{1}{2}$	$8\frac{1}{2}$	$1\frac{1}{2}$	5	13	6	*25*	—	$7\frac{1}{2}$	6	$11\frac{1}{2}$	13	$+\frac{1}{2}$
Valeurs extrêmes		$10\frac{1}{2}$–14	$8\frac{1}{2}$–12	$1\frac{1}{2}$–$4\frac{1}{2}$	4–6	$9\frac{1}{2}$–$14\frac{1}{2}$	$3\frac{1}{2}$–7	Valeurs extrêmes		$5\frac{1}{2}$–8	$3\frac{1}{2}$–$6\frac{1}{2}$	$8\frac{1}{2}$–12	11–15	-4 à $+1$

TABLE XVIII.

Pulmonaires récoltées à Messein. (*Année* 1903.)

Les pieds récoltés étaient situés à 3 mètres au moins les uns des autres. Valeurs en millimètres.

Brévistylées								Brévistémonées						
Nos. des plantes	Nos. des dessins schématiques	Hauteur de l'étamine	Distance du l'anthère à la base du calice	Distance de haut du stigmate a la base de l'anthère	Longueur du style et du stigmate réunis	Hauteur totale du calice	Distance du stigmate au bord du calice	Nos. des plantes	Nos. des dessins schématiques	Hauteur de l'étamine	Distance du haut du stigmate à la partie supérieure de l'anthère	Longueur du style et du stigmate	Hauteur totale du calice	Distance du stigmate au bord du calice
1	—	$11\frac{1}{2}$	$9\frac{1}{2}$	$2\frac{1}{2}$	5	$11\frac{1}{2}$	$4\frac{1}{2}$	*1*	—	7	6	11	$13\frac{1}{2}$	$-\frac{1}{2}$
2	—	13	11	4	5	13	6	*2*	—	$6\frac{1}{2}$	4	$8\frac{1}{2}$	$10\frac{1}{2}$	—
3	42	$13\frac{1}{2}$	$11\frac{1}{2}$	5	$4\frac{1}{2}$	12	6	*3*	—	$6\frac{1}{2}$	6	$10\frac{1}{2}$	$12\frac{1}{2}$	—
4	—	12	10	$2\frac{1}{2}$	$5\frac{1}{2}$	$12\frac{1}{2}$	5	*4*	—	7	5	10	10	+2
5	—	12	10	3	$4\frac{1}{2}$	$12\frac{1}{2}$	6	*5*	—	7	4	9	11	—
6	—	$12\frac{1}{2}$	$10\frac{1}{2}$	$3\frac{1}{2}$	5	12	5	*6*	—	$6\frac{1}{2}$	$4\frac{1}{2}$	9	$10\frac{1}{2}$	$+\frac{1}{2}$
7	—	12	10	3	5	$13\frac{1}{2}$	$6\frac{1}{2}$	*7*	45	7	5	10	8	$+2\frac{1}{2}$
8	—	11	9	2	5	11	4	*8*	—	7	4	9	$11\frac{1}{2}$	$-\frac{1}{2}$
9	—	$11\frac{1}{2}$	$9\frac{1}{2}$	3	$4\frac{1}{2}$	$10\frac{1}{2}$	4	*9*	—	7	4	9	13	$-2\frac{1}{2}$
10	—	12	10	$3\frac{1}{2}$	$4\frac{1}{2}$	$11\frac{1}{2}$	5	*10*	—	7	$5\frac{1}{2}$	$10\frac{1}{2}$	$11\frac{1}{2}$	+1
11	—	12	10	$2\frac{1}{2}$	$5\frac{1}{2}$	$13\frac{1}{2}$	6	*11*	—	$7\frac{1}{2}$	6	$11\frac{1}{2}$	13	$+\frac{1}{2}$
12	—	12	10	4	4	10	4	*12*	40	6	6	10	11	+1
13	36–48	11	9	3	4	10	4	*13*	—	6	6	10	13	−1
14	—	12	10	$2\frac{1}{2}$	$5\frac{1}{2}$	11	$3\frac{1}{2}$	*14*	—	7	$3\frac{1}{2}$	$8\frac{1}{2}$	$11\frac{1}{2}$	−1
15	—	11	9	$2\frac{1}{2}$	$4\frac{1}{2}$	10	$3\frac{1}{2}$	*15*	—	7	$4\frac{1}{2}$	$9\frac{1}{2}$	$10\frac{1}{2}$	+1
16	—	12	10	3	5	14	7	*16*	—	$6\frac{1}{2}$	$5\frac{1}{2}$	10	10	+2
17	—	$11\frac{1}{2}$	$9\frac{1}{2}$	$2\frac{1}{2}$	5	$12\frac{1}{2}$	$5\frac{1}{2}$	*17*	—	7	$5\frac{1}{2}$	$10\frac{1}{2}$	12	$+\frac{1}{2}$
18	—	13	11	4	5	12	5	*18*	—	$6\frac{1}{2}$	$5\frac{1}{2}$	9	$14\frac{1}{2}$	$-3\frac{1}{2}$
19	—	$12\frac{1}{2}$	$10\frac{1}{2}$	$3\frac{1}{2}$	5	15	8	*19*	—	7	$4\frac{1}{2}$	$9\frac{1}{2}$	11	$+\frac{1}{2}$
20	41	11	9	2	$6\frac{1}{2}$	$12\frac{1}{2}$	4	*20*	33	8	$5\frac{1}{2}$	$11\frac{1}{2}$	14	$-\frac{1}{2}$
21	—	$12\frac{1}{2}$	$10\frac{1}{2}$	$2\frac{1}{2}$	6	$13\frac{1}{2}$	$5\frac{1}{2}$	*21*	—	6	$5\frac{1}{2}$	$9\frac{1}{2}$	$11\frac{1}{2}$	—
22	—	13	11	$4\frac{1}{2}$	$4\frac{1}{2}$	$13\frac{1}{2}$	7	*22*	—	$6\frac{1}{2}$	$5\frac{1}{2}$	9	$12\frac{1}{2}$	$-1\frac{1}{2}$
23	—	$13\frac{1}{2}$	$11\frac{1}{2}$	$4\frac{1}{2}$	5	$11\frac{1}{2}$	$4\frac{1}{2}$	*23*	—	7	$4\frac{1}{2}$	$9\frac{1}{2}$	14	$-2\frac{1}{2}$
24	—	13	11	4	5	13	6	*24*	—	7	4	9	9	+2
25	—	$11\frac{1}{2}$	$9\frac{1}{2}$	3	$4\frac{1}{2}$	11	$4\frac{1}{2}$	*25*	—	7	5	10	$11\frac{1}{2}$	$+\frac{1}{2}$
26	—	12	10	$3\frac{1}{2}$	$4\frac{1}{2}$	$10\frac{1}{2}$	4	*26*	—	$7\frac{1}{2}$	$4\frac{1}{2}$	9	$11\frac{1}{2}$	$-\frac{1}{2}$
27	—	11	9	$2\frac{1}{2}$	$4\frac{1}{2}$	$12\frac{1}{2}$	6	*27*	—	$7\frac{1}{2}$	$4\frac{1}{2}$	10	$14\frac{1}{2}$	$-2\frac{1}{2}$
28	—	13	11	$3\frac{1}{2}$	$5\frac{1}{2}$	$10\frac{1}{2}$	3	*28*	—	7	$3\frac{1}{2}$	$8\frac{1}{2}$	$10\frac{1}{2}$	—
29	46	13	11	$3\frac{1}{2}$	$5\frac{1}{2}$	15	$7\frac{1}{2}$	*29*	—	7	5	10	15	−3
30	—	13	11	$4\frac{1}{2}$	$4\frac{1}{2}$	14	$7\frac{1}{2}$	*30*	—	$6\frac{1}{2}$	$4\frac{1}{2}$	9	11	—
31	—	$12\frac{1}{2}$	$10\frac{1}{2}$	$3\frac{1}{2}$	5	13	6	*31*	—	7	5	10	12	—
32	—	$11\frac{1}{2}$	$9\frac{1}{2}$	1	$6\frac{1}{2}$	11	$2\frac{1}{2}$	*32*	43	$7\frac{1}{2}$	6	$11\frac{1}{2}$	$16\frac{1}{2}$	−3
33	—	12	10	3	5	13	6	*33*	—	7	4	9	$10\frac{1}{2}$	$+\frac{1}{2}$
34	—	$12\frac{1}{2}$	$10\frac{1}{2}$	$3\frac{1}{2}$	5	$13\frac{1}{2}$	$6\frac{1}{2}$	*34*	34–39	7	3	8	12	$-2\frac{1}{2}$
35	—	10	8	1	5	$12\frac{1}{2}$	$5\frac{1}{2}$	*35*	—	7	$4\frac{1}{2}$	$9\frac{1}{2}$	$9\frac{1}{2}$	+2
36	—	14	12	5	5	$14\frac{1}{2}$	$7\frac{1}{2}$	*36*	—	7	5	10	$11\frac{1}{2}$	$+\frac{1}{2}$
37	—	$10\frac{1}{2}$	$8\frac{1}{2}$	$2\frac{1}{2}$	4	11	5	*37*	—	7	6	11	$11\frac{1}{2}$	$+1\frac{1}{2}$
Valeurs extrêmes		10–14	8–12	$\frac{1}{2}$–5	4–$6\frac{1}{2}$	10–15	$2\frac{1}{2}$–8	Valeurs extrêmes		6–8	3–6	8–$11\frac{1}{2}$	8–$16\frac{1}{2}$	$-3\frac{1}{2}$ à +2

H. Tableaux des fréquences relatives des diverses longueurs étudiées.

TABLE XIX.

Hauteur totale du calice. Figs. 49 à 52.

Nombre de plantes présentant les diverses hauteurs.

Hauteurs en millimètres	Laxou			Malzéville		Maxéville		Messein			
	Brévistylées	Brévistémonées		Brévistylées	Brévistémonées	Brévistylées	Brévistémonées	Brévistylées		Brévistémonées	
	Sur 25	Sur 25†	Sur 31	Sur 25	Sur 25	Sur 25	Sur 25	Sur 25‡	Sur 37	Sur 25‡	Sur 37
$7\frac{1}{2}$	—	—	—	—	—	—	—	—	—	—	—
8	—	—	—	1	—	—	—	—	—	1	1
$8\frac{1}{2}$	—	—	—	—	—	—	—	—	—	—	—
9	—	—	—	—	—	—	—	—	—	1	1
$9\frac{1}{2}$	—	—	—	—	—	1	—	—	—	—	1
10	—	—	—	1	—	—	—	3	3	2	2
$10\frac{1}{2}$	—	1	1	—	2	—	—	1	3	3	5
11	—	—	—	3	—	2	4	3	5	3	4
$11\frac{1}{2}$	1	—	—	4	2	3	1	3	3	5	8*
12	3	1	1	4	7*	4	4	3	3	1	3
$12\frac{1}{2}$	1	1	1	4	3	3	2	4	6*	2	2
13	6*	6	6	5*	4	9*	4	2	4	3	3
$13\frac{1}{2}$	1	4	7	1	1	—	2	4	4	1	1
14	3	5	5	1	4	2	7*	1	2	2	2
$14\frac{1}{2}$	2	5	8*	—	2	1	—	—	1	1	2
15	4	1	1	1	—	—	1	1	2	—	1
$15\frac{1}{2}$	1	—	—	—	—	—	—	—	—	—	—
16	2	—	—	—	—	—	—	—	—	—	—
$16\frac{1}{2}$	1	—	—	—	—	—	—	—	—	—	1
17	—	1	1	—	—	—	—	—	—	—	—
$17\frac{1}{2}$	—	—	—	—	—	—	—	—	—	—	—
Type le plus fréquent	13 mm.	$14\frac{1}{2}$ mm.		13 mm.	12 mm.	13 mm.	14 mm.	$12\frac{1}{2}$ mm.		$11\frac{1}{2}$ mm.	
Type moyen (pour la station)	$13\frac{3}{4}$ millimètres			$12\frac{1}{2}$ millimètres		$13\frac{1}{2}$ millimètres		12 millimètres			

† En ne tenant pas compte des six premières plantes de la série de 31 plantes brévistémonées (Laxou).

‡ En prenant seulement les 25 premières plantes de la série de 37 plantes de Messein.

* Astérisques, indiquant le plus fort chiffre de chaque colonne.

TABLE XIX *bis*.

Hauteur de l'étamine. Figs. 37 à 40.

Distance du haut de l'anthère au bas du calice.

Hauteurs en millimètres		Nombre de plantes présentant les diverses hauteurs					
		Laxou		Malzéville	Maxéville	Messein	
Brévistémonées	5	—	—	—	—	—	—
	5½	—	—	—	1	—	—
	6	2	2	3	1	3	3
	6½	6	6	7	4	6	7
	7	8*	10*	6	12*	14	22*
	7½	6	9	7	5	1	4
	8	3	4	2	2	1	1
	8½	—	—	—	—	—	—
	Totaux	25	31	25	25	25	37
Brévistylées	9½	—	—	—	—	—	—
	10	—	—	—	—	—	1
	10½	4	—	—	3	—	1
	11	7	—	2	5	4	5
	11½	7*	—	5	7*	4	5
	12	4	—	9*	5	8	10*
	12½	1	—	3	2	3	5
	13	1	—	6	2	4	7
	13½	—	—	—	—	2	2
	14	1	—	—	1	—	1
	14½	—	—	—	—	—	—
	15	—	—	—	—	—	—
	Totaux	25	—	25	25	25	37

TABLE XX.

Hauteur du style et du stigmate au-dessus de l'ovaire. Figs. 41—44.

	Longueurs en millimètres du style et du stigmate réunis	Nombre de plantes présentant les diverses tailles					
		Laxou		Malzéville	Maxéville	Messein	
		Sur 25	Sur 31	Sur 25	Sur 25	Sur 25	Sur 37
Brévistylées	3½	—	—	—	—	—	—
	4	8*	—	3	4	2	3
	4½	7	—	10*	3	7	10
	5	7	—	8	12*	11*	16*
	5½	1	—	1	4	3	5
	6	2	—	3	2	1	1
	6½	—	—	—	—	2	2
	7	—	—	—	—	—	—
Brévistémonées	7½	1	2	1	—	—	—
	8	4	5	4	—	—	1
	8½	5	8	1	6*	2	3
	9	9*	9*	2	3	7*	10
	9½	4	5	5	5	4	5
	10	1	1	7*	4	6	10*
	10½	1	1	1	3	3	3
	11	—	—	4	1	1	2
	11½	—	—	—	2	2	3
	12	—	—	—	1	—	—
	12½	—	—	—	—	—	—

		Laxou	Malzéville	Maxéville	Messein	
Type le plus fréquent (moyenne de fréquence)	Brévistylées	4	4½	5	5	
	Brévistémonées	9	10	8½	10	
		13	14½	13½	15	
Moyenne théorique calculée		4·64	4·74	4·94	5·2 4·95	Moyennes (4·83)
" "		8·86 8·77	9·52	9·72	9·74 9·70	(9·42)

TABLE XXI.

Distance du stigmate au bord du calice. Figs. 45 à 48.

Distance en millimètres du stigmate au bord du calice	Laxou			Malzéville		Maxéville		Messein			
	Brévistylées	Brévistémonées		Brévistylées	Brévistémonées	Brévistylées	Brévistémonées	Brévistylées		Brévistémonées	
	Sur 25	Sur 25	Sur 31	Sur 25	Sur 25	Sur 25	Sur 25	Sur 25	Sur 37	Sur 25	Sur 37
Styles exsertes de + 2½	—	—	—	—	—	—	—	—	—	—	—
+ 2	—	—	—	—	—	—	—	—	—	4	5
+ 1½	—	—	—	—	—	—	—	—	—	—	1
+ 1	—	—	—	—	—	—	1	—	—	3	3
+ ½	—	—	—	—	2	—	3	—	—	5	7*
0	—	—	—	—	5*	—	4	—	—	4	7
Styles inclus de − ½	—	—	—	—	4	—	3	—	—	3	4
− 1	—	2	2	—	2	—	2	—	—	2	2
− 1½	—	2	2	1	4	—	7*	—	—	1	1
− 2	—	6	6	—	5*	—	3	—	—	1	2
− 2½	—	3	3	—	1	—	—	—	1	1	2
− 3	—	4	7*	—	2	—	—	—	1	—	2
− 3½	—	4	5	1	—	1	2	2	2	1	1
− 4	—	—	1	3	—	3	1	5	6	—	—
− 4½	—	3	3	2	—	1	—	3	3	—	—
− 5	1	—	1	5	—	5	—	4	5	—	—
− 5½	2	—	—	3	—	3	—	2	3	—	—
− 6	2	1	1	6*	—	6*	—	5	8*	—	—
− 6½	4	—	—	1	—	3	—	1	2	—	—
− 7	5*	—	—	2	—	3	—	2	2	—	—
− 7½	3	—	—	—	—	—	—	—	3	—	—
− 8	2	—	—	—	—	—	—	1	1	—	—
− 8½	3	—	—	—	—	—	—	—	—	—	—
− 9	2	—	—	1	—	—	—	—	—	—	—
− 9½	1	—	—	—	—	—	—	—	—	—	—
− 10	—	—	—	—	—	—	—	—	—	—	—

TABLE XXII.

Distance du stigmate à l'anthère. Figs. 57 à 60.

Distances en millimètres		Nombre de plantes presentant les diverses distances					Pour 100 25 plantes de chaq. station
		Laxou		Malzéville	Maxéville	Messein	
Brévistylées	0	—		—	—	— —	—
	½	—		—	—	1 1	1
	1	—		—	—	— 2	—
	1½	2		—	3	— —	5
	2	**3**		4	6*	1 1	14
	2½	7*		1	4	2 9*	19
	3	6		7	4	5 6	22
	3½	5		6*	4	4 9*	19
	4	1		4	2	4 4	11
	4½	—		2	1	2 3	5
	5	1		1	1	1 2	4
	5½	—		—	—	— —	—
		25		25	25	25‡ 37	100
Brévistémonées	1	—	—	—	—	— —	—
	1½	—	1	—	—	— —	—
	2	2	2	—	—	— —	2
	2½	0	1	—	—	— —	—
	3	3	5	1	—	1 1	5
	3½	7	8*	5	4	1 2	17
	4	6	6	4	2	5 6	17
	4½	4	5	6*	7*	3 8*	20
	5	2	2	3	4	3 6	12
	5½	—	—	2	6	7 7	15
	6	1	1	2	1	5 7	3
	6½	—	—	—	1	— —	1
	7	—	—	1	—	— —	1
	7½	—	—	—	—	— —	—
		25†	31	25	25	25‡ 37	100

† En ne tenant pas compte des 6 premières plantes de la série des 31 plantes brévistémonées de Laxou.

‡ En ne tenant compte que des 25 premières plantes de la série de 37 plantes de Messein.

I. VARIABILITÉ COMPARATIVE DANS LES QUATRE STATIONS.

TABLE XXIII A.

Variabilité comparée de 25 échantillons observés dans quatre stations différentes.

(Ce tableau, qui donne seulement l'étendue empirique de la variation, a surtout pour but de montrer les dimensions habituelles des plantes des stations considérées, en vue de les comparer aux dimensions des plantes d'autres stations de l'espèce.)

Caractères étudiés	Stations	Variabilité de l'espèce, écart observé	Variabilité des deux types brévistylé et brévistémoné		Distance des sommets de plus grande fréquence. Différence entre les deux types de grande fréquence	Valeur minimum du type		Valeur moyenne de grande fréquence		Valeur maximum du type	
						Brévistylé	Brévistémoné	Du type brévistylé	Du type brévistémoné	Brévistylé	Brévistémoné
Hauteur totale du calice	Laxou	$7\frac{1}{2}$	6	$7\frac{1}{2}$	$1\frac{1}{2}$	11	10	13	$14\frac{1}{2}$	17	$17\frac{1}{2}$
	Malzéville	8	8	5	1	$7\frac{1}{2}$	10	13	12	$15\frac{1}{2}$	15
	Maxéville	$6\frac{1}{2}$	6	5	1	9	$10\frac{1}{2}$	13	14	15	$15\frac{1}{2}$
	Messein	$9\frac{1}{2}$	6	$9\frac{1}{2}$	1	$9\frac{1}{2}$	$7\frac{1}{2}$	$12\frac{1}{2}$	$11\frac{1}{2}$	$15\frac{1}{2}$	17
Hauteur totale des étamines	Laxou	9	$4\frac{1}{2}$	3	4	10	$5\frac{1}{2}$	11	7	$14\frac{1}{2}$	$8\frac{1}{2}$
	Malzéville	8	3	$3\frac{1}{2}$	5	$10\frac{1}{2}$	$5\frac{1}{2}$	12	7	$13\frac{1}{2}$	9
	Maxéville	$9\frac{1}{2}$	$4\frac{1}{2}$	$3\frac{1}{2}$	$4\frac{1}{2}$	10	5	$11\frac{1}{2}$	7	$14\frac{1}{2}$	$8\frac{1}{2}$
	Messein	$8\frac{1}{2}$	$3\frac{1}{2}$	3	5	$10\frac{1}{2}$	$5\frac{1}{2}$	12	7	14	$8\frac{1}{2}$
Hauteur totale du style et du stigmate	Laxou	$7\frac{1}{2}$	3	4	5	$3\frac{1}{2}$	7	4	9	$6\frac{1}{2}$	$10\frac{1}{2}$
	Malzéville	8	3	$4\frac{1}{2}$	$5\frac{1}{2}$	$3\frac{1}{2}$	7	$4\frac{1}{2}$	10	7	$11\frac{1}{2}$
	Maxéville	9	3	$4\frac{1}{2}$	$3\frac{1}{2}$	$3\frac{1}{2}$	8	5	$8\frac{1}{2}$	$6\frac{1}{2}$	$12\frac{1}{2}$
	Messein	$8\frac{1}{2}$	$3\frac{1}{2}$	4	4	$3\frac{1}{2}$	8	5	9	7	12
Distance du stigmate au bord du calice	Laxou	$9\frac{1}{2}$	$5\frac{1}{2}$	6	4	$-4\frac{1}{2}$	-1	-7	-3	-10	$-6\frac{1}{2}$
	Malzéville	$10\frac{1}{2}$	$8\frac{1}{2}$	$4\frac{1}{2}$	4	-1	$+1$	-6	-2	$-9\frac{1}{2}$	$-3\frac{1}{2}$
	Maxéville	9	$4\frac{1}{2}$	6	$4\frac{1}{2}$	-3	$+1\frac{1}{2}$	-6	$-1\frac{1}{2}$	$-7\frac{1}{2}$	$-4\frac{1}{2}$
	Messein	11	$6\frac{1}{2}$	$6\frac{1}{2}$	6	-2	$+2\frac{1}{2}$	-6	—	$-8\frac{1}{2}$	-4
Distance du stigmate à l'anthère	Laxou	$5\frac{1}{2}$	$4\frac{1}{2}$	$5\frac{1}{2}$	1	1	1	$2\frac{1}{2}$	$3\frac{1}{2}$	$5\frac{1}{2}$	$6\frac{1}{2}$
	Malzéville	6	4	5	$1\frac{1}{2}$	$1\frac{1}{2}$	$2\frac{1}{2}$	3	$4\frac{1}{2}$	$5\frac{1}{2}$	$7\frac{1}{2}$
	Maxéville	6	4	4	$2\frac{1}{2}$	1	3	2	$4\frac{1}{2}$	5	7
	Messein	$6\frac{1}{2}$	$5\frac{1}{2}$	4	$1\frac{1}{2}$	—	$2\frac{1}{2}$	3	$4\frac{1}{2}$	$5\frac{1}{2}$	$6\frac{1}{2}$

TABLE XXIII B.

Comparaison des valeurs obtenues dans quatre stations différentes.

A. Plantes brévistémonées.

Caractère étudié	Laxou			Malzéville			Maxéville			Messein		
	M	σ	$100\frac{\sigma}{M}$	M	σ	$100\frac{\sigma}{M}$	M	σ	$100\frac{\sigma}{M}$	M	σ	$100\frac{\sigma}{M}$
Hauteur totale du calice	13·72	1·076	7·83	12·64	1·100	8·70	12·64	1·156	9·14	11·86	1·729	14·58
Hauteur totale des étamines	7·11	0·549	7·72	6·96	0·582	8·36	7·00	0·548	7·83	6·91	0·833	12·06
Hauteur totale du style	8·77	0·682	7·78	9·52	1·024	10·76	9·72	1·021	10·50	9·70	0·874	9·01
Distance du stigmate au bord du calice	−2·95	1·152	—	−1·10	1·029	—	−1·10	1·288	—	−0·16	1·489	—
Distance du stigmate à l'anthère ...	3·66	0·936	25·57	4·52	0·932	20·62	4·76	0·626	13·15	4·88	0·817	16·74

B. Plantes brévistylées.

Caractère étudié	Laxou			Malzéville			Maxéville			Messein		
	M	σ	$100\frac{\sigma}{M}$	M	σ	$100\frac{\sigma}{M}$	M	σ	$100\frac{\sigma}{M}$	M	σ	$100\frac{\sigma}{M}$
Hauteur totale du calice	13·86	1·375	9·92	12·10	1·342	11·17	12·44	1·061	8·53	12·27	1·383	11·27
Hauteur totale des étamines	11·48	0·806	7·02	12·12	0·621	5·12	11·68	0·847	7·25	12·09	0·884	7·31
Hauteur totale du style	4·64	0·592	12·76	4·82	0·564	11·70	4·94	0·554	11·22	4·96	0·574	11·57
Distance du stigmate au bord du calice	−7·22	1·167	16·16	−5·32	1·392	26·17	−5·64	0·878	15·57	−5·32	1·319	24·79
Distance du stigmate à l'anthère ...	2·84	0·771	27·15	3·30	0·800	24·24	2·82	0·942	33·40	3·14	1·011	32·20

TABLE XXIV.

Distance du stigmate au bord du calice.

	Laxou	Malzéville	Maxéville	Messein
Styles inclus	100 par 100	72	69	38
Styles égaux au calice	0	20	15	19
Styles ex.	0	8	16	43

Longueur du style (l) relativement au calice (l'). $l' - l = d$.

Sur 100 styles il y a

	Laxou	Malzéville	Maxéville	Messein
$l < d$	92	60	60	55
$l = d$	8	20	20	13
$l > d$	0	20	20	32

Donc, pour les brévistylées comme pour les brévistémonées le style a la plus grande capacité de croissance à Messein par rapport à la capacité de croissance du calice.

L. CARACTÈRES GÉNÉRAUX DES RACES GÉOGRAPHIQUES OBSERVÉES.

Si l'on compare les polygones de fréquence obtenus avec 25 plantes d'une localité (Messein) et ceux qu'on obtient avec 37 plantes du même lieu, on constate que les sommets et les limites latérales de ces polygones correspondent sensiblement aux mêmes points de la ligne des abscisses.

La même conclusion est exacte pour les polygones obtenus à Maxéville avec les récoltes simultanées de 25 plantes et 100 plantes.

Malgré la faiblesse relative du chiffre 25, il n'y a donc pas de causes d'erreurs suffisantes, pour nous interdire de tirer des conclusions relatives aux races géographiques de la Pulmonaire des 4 localités comparées. Ces comparaisons portent sur les points suivants:

1. *Hauteur totale du calice.* 2. *Distance du stigmate au bord du calice.* 3. *Hauteur de l'étamine.* 4. *Distance du stigmate à l'anthère.*

Si on compare soit les chiffres des Tables XXIII A et B soit les graphiques (Figs. 37—48; 49—52; 57—60) on voit tout de suite qu'il y a des variations assez accusées dans les quatre stations étudiées.

Ce sont les plantes brévistémonées de Messein qui ont montré le calice le plus variable, avec tendance vers les petites tailles: la plus fréquente étant seulement d'une longueur de 11 mill. $\frac{1}{2}$.

Si l'on suppose que la grande capacité de croissance du calice correspond à une vigueur plus grande de la fleur on peut penser que les autres parties de la fleur seront peut-être influencées dans le même sens. D'autre part si la capacité de croissance du calice est faible cela peut provenir d'un arrêt de développement de l'organe. Dans cette hypothèse un organe voisin, le style par exemple, pourrait bénéficier de cet arrêt de développement et exagérer lui-même sa propre capacité de croissance.

Si on consulte la Table (XXIII B) on voit que, parmi les 4 types brévistylés, c'est justement le type brévistylé Laxou qui a le calice du type moyen le plus grand tandis que c'est lui qui a le style du type moyen le plus petit.

De même, parmi les 4 types brévistémonés c'est le type brévistémoné Messein qui a sensiblement le style du type moyen le plus grand et le calice du type moyen le plus petit.

Les chiffres de la Table XXIII B semblent bien démontrer ce fait d'ailleurs visible aussi sur les mensurations individuelles *faites sur des centaines d'échantillons :*

La longueur du calice et la longueur du style sont deux quantités qui varient en sens inverse, aussi bien chez les fleurs brévistylées que chez les plantes brévistémonées, en passant d'une race à une autre.

Il était naturel de penser que cela entraîne cette autre conclusion :

Les types de styles les plus inclus appartiennent aux fleurs ayant les plus grands calices, et inversement les types de styles les plus exsertes appartiennent aux fleurs ayant les plus petits calices.

Il en est ainsi dans beaucoup de cas, et la comparaison des types moyens est démonstrative à cet égard :

Distance du stigmate au bord du calice.

Localités	Pour le type de grande fréquence de chaque station			Pour le type moyen		
	Brévistylées	Brévistémonées	Différence entre les deux types	Brévistylées	Brévistémonées	Différence
Laxou ...	−7 mill.	−3 mill.	4	−7·22	−2·95	4·27
Maxéville	−6 „	−1½ „	4½	−5·64	−1·10	4·54
Malzéville	−6 „	−2 ou 0	4 ou 6	−5·32	−1·10	4·20
Messein...	−6 „	+½ ou 0	6½	−5·32	−0·16	5·16

Il y a évidemment, à Laxou, tendance à la production d'une race à grand calice, à style court et très inclus dans le calice ; tandis qu'à Messein on voit qu'il y a une tendance très accentuée à la production d'une race à calice court et à style long exserte. Voyez Table XIV.

Ceci d'ailleurs est contrôlé aussi par les mensurations individuelles. Nous avons noté, en effet, que chez certaines fleurs il y a une sorte de nanisme floral, c'est-à-dire une réduction proportionnelle des diverses parties.

On trouve donc ici une démonstration que le type géographique en formation est au moins partiellement fixé.

On trouvera par exemple à Messein des fleurs à styles très exsertes de deux sortes :

1er cas. Celles qui ont un calice plus court que la moyenne du type et qui ont un style plus long que la moyenne du type.

C'est une race en formation, ne tenant peut-être sa particularité que comme variation individuelle.

2e cas. Celles qui ont un calice plus grand et un style plus grand, ou bien encore un calice plus petit et un style plus petit, que la moyenne du type.

Celles-là sont des races fixées, au moins partiellement : elle tiennent la particularité qui les caractérise, d'une influence héréditaire.

Nous pouvons comprendre la formation de ces races en constatant que l'arrêt de développement du calice entraîne le plus grand développement du style ou inversement.

On peut supposer que dans le bouton floral le temps qui sépare le début de la croissance des deux organes est un facteur important qui modifie leur capacité de croissance, et comme conséquence amène la formation de races spéciales.

De même pendant le grand développement de la fleur, au moment de son épanouissement il suffit d'une influence du milieu physique (pluie...température...) pour expliquer la variation des proportions relatives des deux organes ; ceux-ci ne se trouvant pas à la même période de leur évolution.

Types moyens théoriques calculés (*Moyennes arithmétiques*).

Conclusions relatives à la variabilité dans les quatre localités étudiées.

1. Le style a une capacité de croissance moyenne qui est plus forte et va en s'accroissant dans l'ordre suivant : Laxou, Malzéville, Messein, Maxéville.

Soit 100 la capacité de croissance moyenne du style des brévistémonées à Laxou ; elle sera 110·83 à Maxéville, soit 10·8 % en plus.

Soit 100 la capacité de croissance moyenne du style des brévistylées à Laxou ; elle sera 106·46 à Maxéville, soit 6·4 % en plus.

2. Les tailles extrêmes du style qui ont été observées sont plus grandes à Maxéville. Mais il est vrai que le nombre des échantillons observés a été plus grand qu'ailleurs. L'accroissement se fait dans le même ordre : Laxou, Malzéville, Messein, Maxéville, pour les Brévistémonées.

C'est donc à Maxéville qu'on a trouvé le plus grand style et à Laxou qu'on a trouvé le plus petit.

Il en est de même pour les styles ; ils correspondent aux plus grandes et aux plus petites étamines.

3. Les tailles moyennes du calice sont au contraire plus grandes à Laxou (13·86 et 13·72).

La variation semble se faire en sens inverse du style.

Soit 100 la capacité de croissance moyenne du calice des brévistémonées à Laxou ; elle sera 90·53 à Maxéville, soit 9·5 en moins ; et 86 à Messein, soit 14 % en moins.

Soit 100 la capacité de croissance moyenne du calice des brévistylées à Laxou ; elle sera 89·86 à Maxéville, soit 10·2 % en moins ; et 88·72 à Messein, soit 11·3 % en moins.

4. Le type moyen du calice est remarquablement fixé dans chaque station pour les deux types brévistylés et brévistémonés.

	Types moyens (calice)		Types moyens (style)	
	Brévistémonées	Brévistylées	Brévistémonées	Brévistylées
Laxou	13·72 mm.	13·86 mm.	8·77 mm.	4·64 mm.
Malzéville	12·64 „	12·10 „	9·52 „	4·82 „
Messein	11·86 „	12·27 „	9·70 „	4·96 „
Maxéville	12·64 „	12·44 „	9·72 „	4·94 „

La différence relativement plus accusée entre les deux chiffres 11·86 et 12·27 de Messein provient de ce fait que à Messein le calice des échantillons du type des brévistémonées a été trouvé très variable dans les échantillons observés (8 à 16 mill.) tandis que le type des brévistylées est plus concentré autour du type moyen (10 à 15).

C'est bien là ce qui explique la faiblesse du chiffre 11·8. Le chiffre 12·28 est tout-à-fait normal.

5. Le pistil a ordinairement une longueur un peu moindre que l'étamine du type inverse, surtout en ce qui concerne les pistils des fleurs brévistémonées :

	Types moyens théoriques (Valeurs en millimètres)				Différences entre le pistil et les étamines des types inverses	
	Brévistémonées		Brévistylées			
	Pistil	Étamines	Pistil	Étamines	Étamines et pistils de grande taille	Étamines et pistils de petite taille
Laxou	10·77	7·11	6·64	11·48	0·71, soit 6 %	0·47, soit 6·6 %
Malzéville	11·52	6·96	6·82	12·12	0·60 „ 4·9 %	0·14 „ 1·9 %
Messein	11·70	6·91	6·96	12·09	0·39 „ 3·2 %	0·05 „ 0·7 %
Maxéville	11·72	7	6·94	11·68	0·04 „ 0·3 %	0·06 „ 0·8 %
Moyennes	11·43	6·99	6·82	11·84		

Dans les échantillons qui ont été passés en revue, et mesurés avec attention, le pistil d'une fleur brévistylée et l'étamine d'une fleur brévistémonée d'une même station sont ordinairement, assez rigoureusement, d'une même taille moyenne. Le pistil d'une fleur brévistémonée et l'étamine d'une fleur brévistylée d'une même station sont, seulement approximativement, de même taille moyenne, l'étamine étant ordinairement un peu plus grande, au maximum de $\frac{1}{10}$.

6. La distance du stigmate à l'anthère est très différente chez les brévistémonées et chez les brévistylées. Elle est plus faible de $\frac{1}{3}$ environ chez les brévistylées. C'est, comme on le voit pour *P. officinalis* une conclusion semblable à celle qui a été obtenue par Darwin avec 10 échantillons de *P. angustifolia* :

	Brévistémonées	Brévistylées	Différence des deux types	Moyennes des 2 types
Laxou ...	3·66 mm.	2·84 mm.	0·82, soit 28 °/₀	3·25
Malzéville ...	4·52 „	3·30 „	1·20 „ 35 °/₀	3·91
Messein ...	4·88 „	3·14 „	1·74 „ 47 °/₀	4·01
Maxéville ...	4·76 „	2·82 „	1·94 „ 68 °/₀	3·79
Chiffre moyen	4·45 „	3·02 „	1·42, soit 47 °/₀	3·73

Si on représente par 100 la distance moyenne du stigmate à l'anthère chez les fleurs brévistylées, la distance est de 147 chez les brévistémonées, soit une différence de 47 p. 100.

D'autre part cette différence est plus faible chez les deux types de la station de Laxou. Elle est plus forte chez les types de Messein et Malzéville, et atteint 68 °/₀ à Maxéville.

Si on représente par 100 le chiffre de la moyenne des 2 types : 3·73, les valeurs correspondants des différentes stations sont :

Laxou 87·1
Malzéville 105
Messein 107·5
Maxéville 101·6.

Les stations de Messein et Laxou diffèrent donc de près de 20 p. 100, soit $\frac{1}{5}$.

Variabilité et fixité de l'hétérostylie de la Pulmonaire.

Si l'on veut apprécier la *variabilité et la fixité relatives de l'espèce,* dans les quatre stations, il y a lieu de ne pas s'en tenir à la comparaison des types de grande fréquence fournis par les graphiques. Il faut calculer le type moyen théorique, qui est obtenu en faisant la moyenne arithmétique des diverses longueurs observées.

Après avoir vu défiler des tailles extrêmes différentes, et de fréquence respective assez variable, on est surpris de constater, pour l'étamine et pour le pistil, le peu de différence présentée par les moyennes théoriques calculées, dont les chiffres figurent à la page 446.

	Hauteur de l'étamine au-dessus de la base du calice		Hauteur du pistil au-dessus de la base du calice	
	Brévistylées	Brévistémonées	Brévistylées	Brévistémonées
Écart des extrêmes	5·6 %	3 %	8 %	7 %
Amplitude des variations extrêmes observées par rapport au chiffre de la moyenne générale	3 à 4 %	2 %	3 à 4 %	4—5 %

Il ressort de ces chiffres

(1) Que l'amplitude ordinaire de la variation est relativement faible.

(2) Que l'étamine est moins variable de longueur que le style.

(3) Que les fleurs brévistémonées semblent un peu moins variables que les fleurs brévistylées.

La première conclusion explique pourquoi l'hétérostylie peut se maintenir malgré la multiplicité des tailles observées, et malgré le fait de l'existence de petites races géographiques très localisées. On peut supposer que si l'hétérostylie a été perdue par certaines espèces de pulmonaires c'est que, à un moment donné l'amplitude de la variation moyenne théorique a été très supérieure à 10 % et n'est pas rentrée ensuite dans les limites de ce chiffre.

Darwin avait émis l'hypothèse que certaines Pulmonaires présentaient peut être une hétérostylie mal fixée. Ce cas particulier explique en outre que *l'évolution graduelle vers un type non hétérostylé semble ne pas exister à Nancy.* Pour que l'hétérostylie disparaisse chez un type hétérostylé habituellement, il ne suffit pas qu'une cause brusque affole le type, et le rende très variable, il faut que le type moyen lui-même soit très modifié.

Si l'on étudie la variation de l'hétérostylie de la Pulmonaire on voit que la variabilité spécifique doit s'accuser par la variation de la position de l'axe de plus grande fréquence qui peut être repoussée à droite ou à gauche.

Il faut constater que la conclusion finale n'est pas celle que pourrait laisser entrevoir les mesures biométriques prises isolément sur quelques échantillons seulement :

L'hétérostylie de Pulmonaria off. semble très peu variable en tant que caractère fondamental de l'espèce. Elle se manifeste avec des modalités très variées, mais elle est bien fixée, au moins dans les localités observées.

La variation la plus importante observée dans les diverses stations consiste dans les différences relatives de développement du calice et du style : *Il y a en*

certains points de la station de Messein une race de Pulmonaire officinale à style très exserte par rapport au calice et, au contraire, *la station de Laxou offre une race à style très inclus.*

On peut dire de même que certaines races tendent à présenter chez les deux types de fleurs *une distance très variable du stigmate à l'anthère.* Cette distance étant beaucoup plus grande chez les fleurs dolichostylées.

CONCLUSIONS GÉNÉRALES.

1. Quelles que soient les variations individuelles de la taille de *Pulmonaria officinalis*, les types morphologiques et les dimensions des organes floraux ne sont pas modifiés par les variations de la taille des tiges. Table IV, page 411, et Table VI, page 413.

La distance du stigmate à l'anthère, signalée par Darwin comme variable chez *Pulmonaria angustifolia*, est aussi très variable chez *P. officinalis*; mais cette variation ne dépend pas de la taille des plantes. Fig. 3, page 415.

D'une façon générale, chez les petites plantes les dimensions des organes floraux subissent des variations un peu plus grandes que chez les grandes plantes. Chez celles-ci le type est plus concentré autour des dimensions moyennes et fréquentes.

2. Les grandes tiges présentent un plus grand nombre de fleurs : soit, en moyenne, 1560 fleurs pour 100 grandes plantes, contre 1360 fleurs pour 100 petites plantes. Le nombre moyen des fleurs par individu n'est pas très différent pour les plantes brachystylées, et les plantes dolichostylées : on trouve quelquefois, chez les premières, un nombre un peu plus grand de fleurs (Fig. 5, page 423), mais Hildebrand a généralisé à tort qu'il en était toujours ainsi. Il y a des localités où le nombre moyen de fleurs est tout à fait le même pour les plantes brachystylées ou brévistylées et pour les plantes brévistémonées ou dolichostylées. Fig. 7 et Fig. 10, page 454.

La floraison plus ou moins abondante ne semble donc pas en rapport avec l'hétérostylie.

3. Si on explore une surface de 1 ou 2 hectares des bois des environs de Nancy, en prenant toutes les Pulmonaires rencontrées, les plantes brévistylées paraissent plus nombreuses, d'environ $\frac{1}{10}$ à $\frac{1}{9}$, que les plantes dolichostylées ou brévistémonées. Mais ce calcul présente une certaine incertitude; le résultat obtenu dépend peut-être de l'étendue prise comme unité de surface, ou même de la dernière période du rythme météorologique. On peut admettre, en effet, que les deux types ne sont pas tout à fait dans les mêmes conditions, pour résister, par exemple, à des pluies qui peuvent à un moment donné, produire sur les deux types une coulure inégalement défavorable.

4. Les Tables X et XI (pp. 420, 421) montrent très nettement la comparaison des dimensions des fleurs brévistylées et des fleurs brévistémonées. Les différences

sont peu nombreuses. Les deux types oscillent autour des mêmes dimensions moyennes, et les chiffres extrêmes observés dans une centaine d'échantillons sont assez peu différents. Il y a pourtant une particularité très intéressante à noter, c'est l'inégalité de la distance entre le stigmate et l'anthère. Elle est chez les brévistylées plus faible d'au moins $\frac{1}{3}$ de ce qu'elle est chez les brévistémonées. Dans certaines stations ces deux distances sont entre elles comme 100 est à 172. Pour la moyenne des chiffres de quatre stations des environs de Nancy, on trouve, pour les brévistylées une valeur moyenne de 3·02mm. et pour les brévistémonées 4·45mm., soit, par rapport au chiffre le plus faible des deux, 47 p. 100 en plus (voir page 447. Figs. 14, 55, 57 à 60).

Il est curieux de constater l'absolue fixité de l'hétérostylie avec un caractère aussi variable que celui que nous venons d'examiner: le libre croisement maintient la constance des dimensions moyennes du style et de l'étamine, sans réaliser la constance de la différence des longueurs de ces organes dans chaque fleur (voir page 446 et les graphiques, Figs. 57—60, 37—40, 41—44).

5. L'influence du climat de l'année ne semble pas modifier la capacité de croissance moyenne, et les capacités de croissance extrêmes des diverses parties de la fleur hétérostylée. Tables XIV *a* et XIV *b*, pp. 424—29, et Figs. 61—63, 53—56.

6. Si on compare des Pulmonaires de quatre stations différentes (Laxou, Malzéville, Maxéville, Messein) on voit qu'il y a des différences assez accusées. Fig. 6, Tables XXIII A et B, pages 441—2.

La longueur du calice et la longueur du style sont deux quantités qui varient en sens inverse aussi bien chez les plantes brévistylées que chez les plantes brévistémonées.

Certaines stations, comme Messein par exemple, peuvent posséder des races de Pulmonaires à styles exsertes du calice, tandis que d'autres (Laxou) donnent des fleurs à styles profondément inclus dans le calice. Il est à remarquer d'ailleurs que les fleurs brévistylées de Messein présentent aussi des étamines moins incluses que celles de Laxou (voir page 451 les dessins des types moyens théoriques, et les types extrêmes de ces deux localités).

La distance du stigmate à l'anthère n'est pas la même dans les diverses stations: pour les brévistémonées elle varie de 3·66 mm. à 4·80mm.; pour les brévistylées elle varie de 2·74 à 3·34. Cela représente une différence qui, par rapport au chiffre le plus faible, peut atteindre 31 °/₀ pour les premières, et 21 °/₀ pour les secondes, page 447.

Ainsi nous pouvons conclure que dans chaque station il est possible de trouver des sortes de races géographiques locales orientées vers une modification spéciale des proportions des divers organes.

Pour ce qui est du phénomène de l'hétérostylie il constitue, chez *Pulmonaria officinalis*, un caractère specifique très peu variable.

Cette hétérostylie est plus ou moins accusée chez les divers individus, et nous avons même trouvé une plante où la distance du stigmate à l'anthère était presque

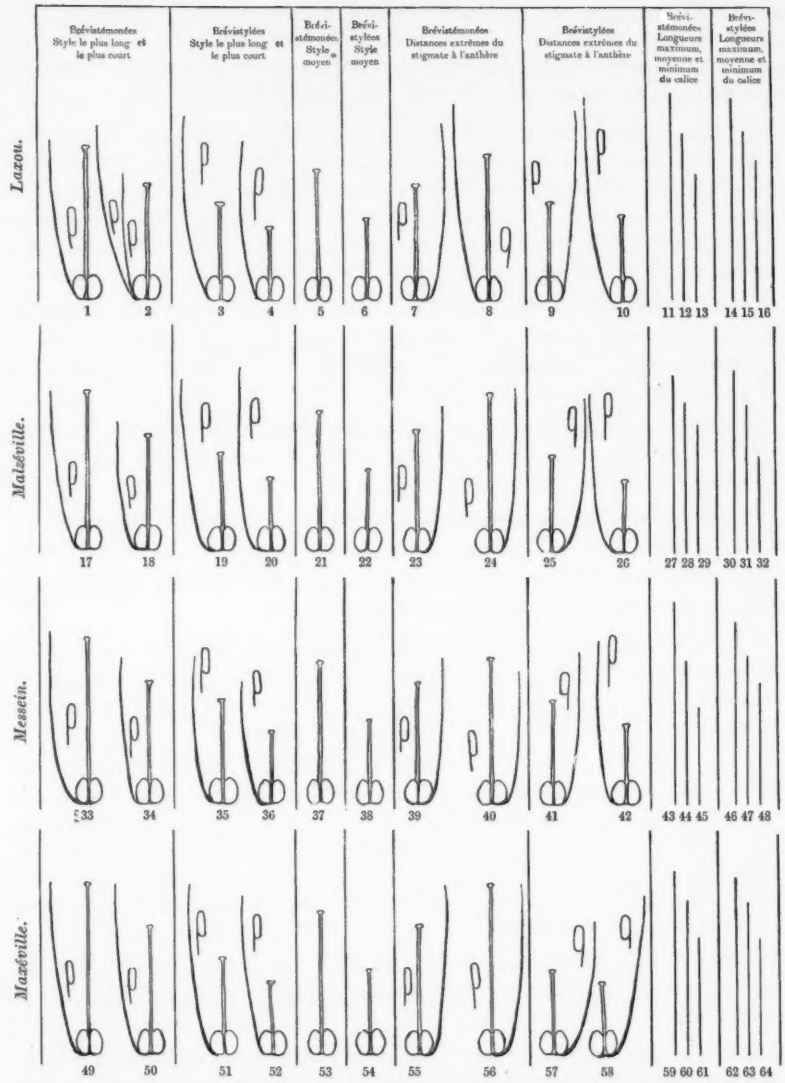
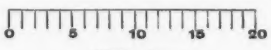

Brévistémonées Style le plus long et le plus court
Brévistylées Style le plus long et le plus court
Brévistémonées Style moyen
Brévistylées Style moyen
Brévistémonées Distances extrêmes du stigmate à l'anthère
Brévistylées Distances extrêmes du stigmate à l'anthère
Brévistémonées Longueurs maximum, moyenne et minimum du calice
Brévistylées Longueurs maximum, moyenne et minimum du calice
Laxou.
Malzéville.
Messein.
Mazéville.
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16
17 18 19 20 21 22 23 24 25 26 27 28 29 30 31 32
33 34 35 36 37 38 39 40 41 42 43 44 45 46 47 48
49 50 51 52 53 54 55 56 57 58 59 60 61 62 63 64

FIG. 6. Schémas représentant les types extrêmes ou caractéristiques de Pulmonaires pour les quatre stations étudiées. (Voyez Tables XV, XVI, XVII, XVIII, pp. 432—435.)



nulle*. Mais, malgré les types de fleurs si différents, les types moyens calculés sont d'une remarquable fixité pour les races des quatre localités étudiées (voir page 446). Et pourtant la loi de fréquence est légèrement modifiée dans beaucoup de cas (voir les graphiques, 37—52 et 57—60, pages 457—8).

7. L'ensemble du travail donne des renseignements sur la nature des courbes relatives à la variation de longueur des organes des deux types d'une fleur hétérostylée. Les graphiques 53 à 56, établis avec des chiffres suffisamment nombreux, et comparés aux autres graphiques du mémoire, permettent de schématiser les résultats en donnant les courbes qui synthètisent les nombreuses mensurations biométriques de ce travail.

a. En ce qui concerne *les longueurs du style et de l'étamine* des deux types de fleurs, les courbes de fréquence de ces longueurs sont des courbes hyperbinomiales. Une certaine asymétrie pourtant se manifeste sur les courbes du style dont l'axe tend à être rejeté vers la gauche. Fig. 53.

Les sommets de grande fréquence sont plus surélevés pour les styles des fleurs brévistylées que pour les autres, Fig. 53; c'est l'inverse pour les sommets de fréquence des étamines. Figs. 39 et 43, 38 et 42.

Sur 1000 échantillons de plantes brévistémonées il y en a environ 450 présentant des étamines moyennes, et 275 présentant des styles moyens.

Sur 1000 échantillons de plantes brévistylées on trouve environ 325 plantes à étamines moyennes, et 400 plantes à styles moyens.

Si on ajoute deux millimètres pour la hauteur de l'ovaire, aux longueurs les plus fréquentes des *styles*, on obtient la hauteur des *pistils* les plus fréquents des deux types (7 et $11\frac{3}{4}$ mm.). Les hauteurs des étamines des types inverses sont justement aussi 7 et $11\frac{3}{4}$ pour les types les plus fréquents.

Le "type théorique," des Pulmonaires de Maxéville, vérifie exactement cette relation. Les types des autres stations peuvent s'en écarter seulement au maximum de 6°/₀ (Laxou), (voir page 446), et c'est généralement l'étamine qui est un peu plus grande que le style de la fleur inverse. Cette fixité assez remarquable du type moyen théorique de l'étamine et du pistil s'oppose à la variabilité si considérable, signalée précédemment, pour les valeurs moyennes théoriques de la distance du stigmate à l'anthère.

On voit, par les graphiques, que la longueur du pistil comme la longueur de l'étamine peuvent varier de 5 à 15 millimètres. Voyez aussi Tables XXIII A et B, pages 441—2.

L'étamine longue et le style long ont plus de possibilité de variation (9 à 15 et 7 à 14) que l'étamine courte et le style court (5 à 9 et 4 à 8).

* Le présent travail n'en démontre pas moins qu'il n'existe pas à Nancy de "type général" de Pulmonaire ayant le style et les étamines de même taille. Plusieurs auteurs français admettent l'existence de ce troisième type général. Il est possible que quelques types extrêmes, analogues à celui qui est mentionné ici, aient éte observées. Mais existe-t-il une localité où ce type est fixé?

b. La distance du stigmate à l'anthère est différente dans les deux types brachystylés et dolichostylés, et plus grande dans ce dernier, Fig. 55. Les deux courbes de fréquence sont binomiales, d'égale hauteur ; la distance de leurs axes de fréquence est de 0·8 à 2 millimètres, soit de 25 à 70 °/₀ de la valeur totale.

c. La distance du stigmate au calice est représentée par deux courbes binomiales normales, Fig. 56. La courbe des brévistémonées étant plus haute, avec axe de grande fréquence sur −1, c'est-à-dire que le stigmate est inclus dans le calice de 1 millimètre. La courbe des fleurs brévistylées est moins haute et par conséquent plus étalée. Elle indique une très grande variabilité, depuis 0 jusque −12, c'est-à-dire que le stigmate peut affleurer à la pointe des lobes du calice ou rester inclus de 12 millimètres. L'axe de grande fréquence est sur − 6.

d. La hauteur du calice est représentée par une seule courbe pour les deux types de fleurs. L'hétérostylie n'influence donc pas les dimensions de cet organe, d'ailleurs très variable. Fig. 54.

La courbe est probablement binomiale avec une région à droite de l'axe de plus grande fréquence, qui est presque toujours irrégulière et tend à se surélever avec affaissement immédiat.

C'est la seule des courbes qui soit moins précisée et moins certaine. Cette incertitude résulte peut-être de ce fait que tous les lobes du calice ne sont pas toujours égaux, ce qui donne un flottement dans les mensurations. Dans ce cas en effet le chiffre exact à retenir serait la moyenne des 5 chiffres fournis par les 5 lobes. Or ce calcul n'était pas réalisable rapidement et n'a pas été fait. On prenait le chiffre le plus fort. Comme on le voit c'est dans les grandes tailles que survient la perturbation de la courbe. Or nous avons observé que les calices à dents inégales étaient surtout fréquents dans les calices très développés.

L'EXPLICATION DES GRAPHIQUES.

Figs. 7, 12, 13 à 18. Réprésentation graphique des chiffres du Tabl. VIII. Maxéville, 3ᵉ lot, 1903, pp. 417—424; Comparaison des plantes brévistylées —— et des plantes brévistémonées - - - - -.

Figs. 8, 9, 9 *bis* (Distance du stigmate au bord du calice), 10, 11. Comparaison graphique entre les plantes de grande taille - - - - et les plantes de petite taille —— récoltées à Maxéville 1902. Toutes les plantes brévistémonées. Voyez Tabl. V, p. 412.

Figs. 19—27. Réprésentation graphique des chiffres des Tabl. IX et XII. Voyez pp. 419, Messein, et 425, Maxéville, 1ᵉʳ lot, 1903.

Figs. 28—36. Réprésentation graphique des chiffres du Tabl. XII. Voyez p. 425. Fig. 31 et Fig. 35, *au lieu de* inclu *lisez* exserte.

Figs. 37—48. Réprésentation graphique de l'influence de la station. Figs. 37—40, Hauteur totale des étamines, p. 437; Figs. 41—44, Longueur du style, p. 438; Figs. 45—48, Distance du stigmate au bord du calice, p. 439; Figs. 46 et 47, *au lieu de* inclus *lisez* exsertes; Figs. 49—52, Hauteur totale du calice, p. 436; Figs. 57—60, Distance du stigmate à l'anthère, p. 440.

Figs. 53—63. Réprésentation graphique de l'influence du climat annuel. Figs. 53—56, Nombres totaux à Maxéville en 1903, p. 425; Figs. 61—63, Nombres totaux à Maxéville en 1902, p. 428.

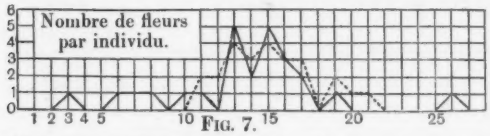
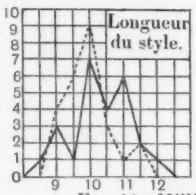


FIG. 7.



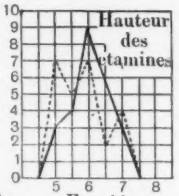


FIG. 10. Millimètres. FIG. 11.

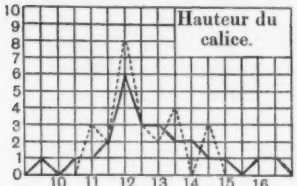
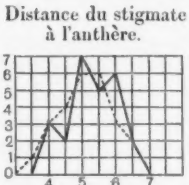
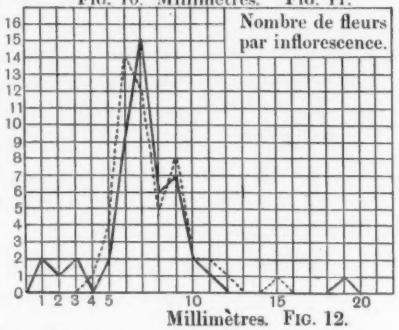
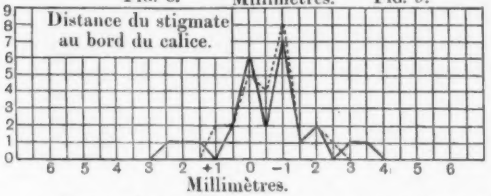


FIG. 8.



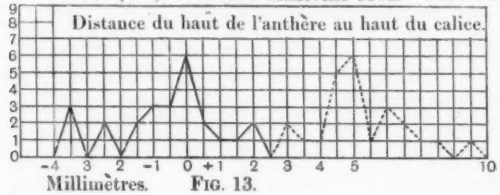
Millimètres. FIG. 9.



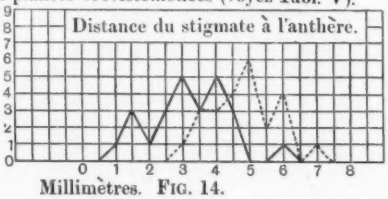


Millimètres. Millimètres. FIG. 12.

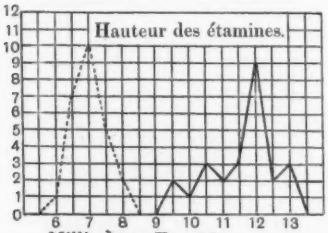
Comparaison graphique entre les plantes de grande taille (- - - - -) et les plantes de petite taille (——) récoltées à Maxéville 1902.—Toutes les plantes brévistémonées (voyez Tabl. V).



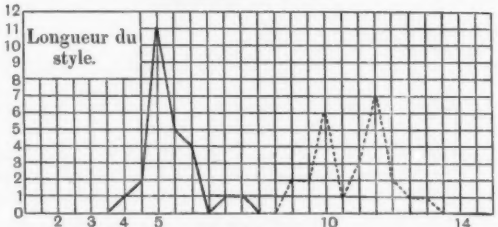
Millimètres. FIG. 13.



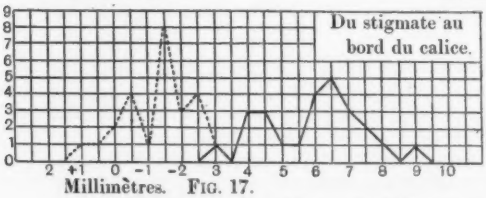
Millimètres. FIG. 14.



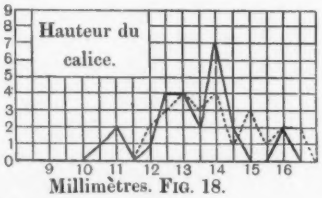
Millimètres. FIG. 15.



Millimètres. FIG. 16.



Millimètres. FIG. 17.



Millimètres. FIG. 18.

Représentation graphique des chiffres du Tabl. VIII. Plantes brévistylées —— Plantes brévistémonées - - - - - - Un degré de l'échelle verticale = un individu.

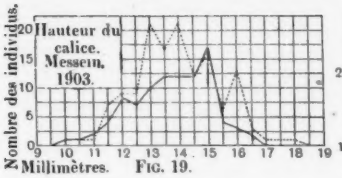


FIG. 19.

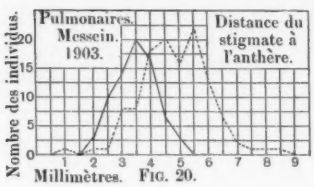


FIG. 20.

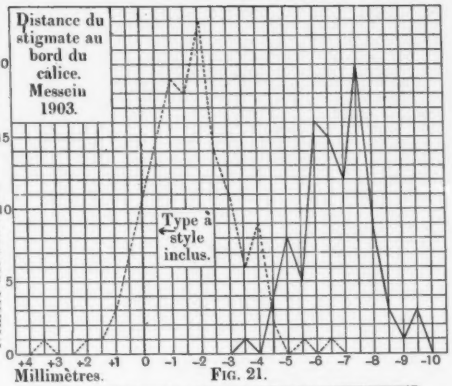


FIG. 21.

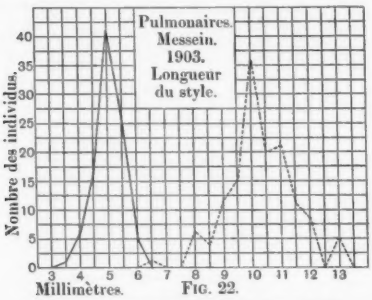


FIG. 22.

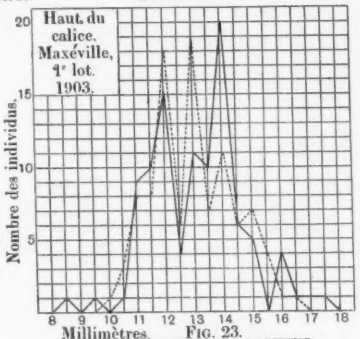


FIG. 23.

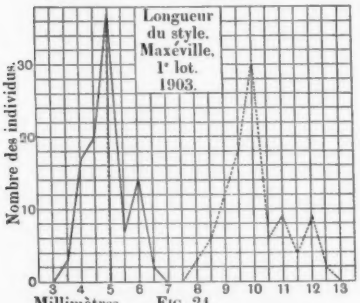


FIG. 24.

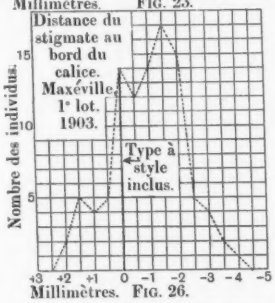


FIG. 26.

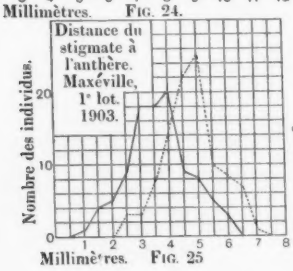


FIG. 25

Brévistylées

Brévistémonées

Voyez Tabl. IX et XII.

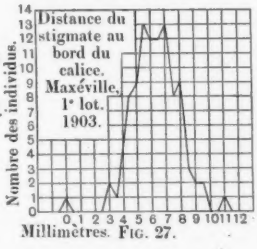
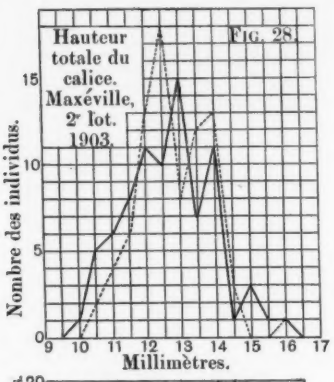


FIG. 27.



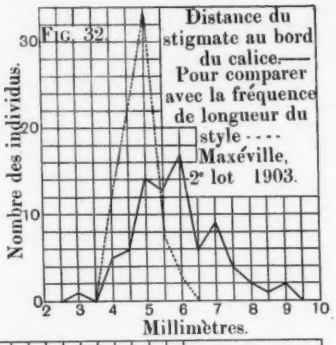
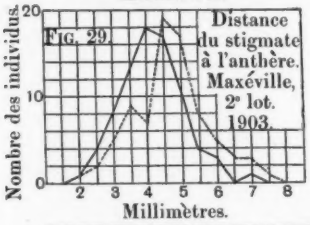
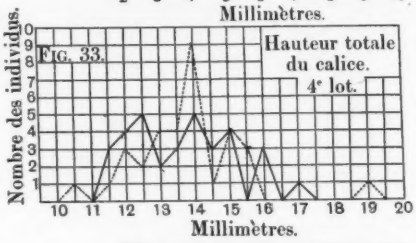
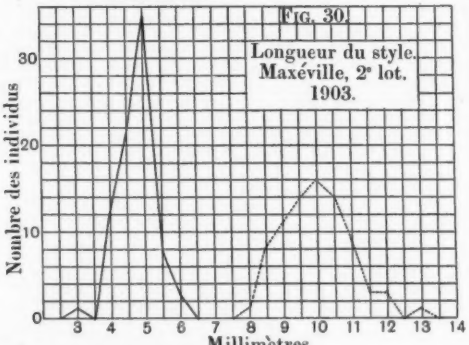
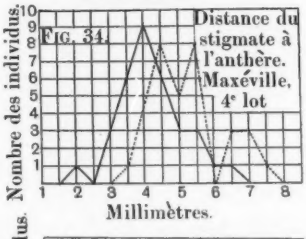


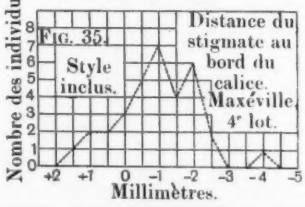
Fig. 32.
Distance du stigmate au bord du calice.— Pour comparer avec la fréquence de longueur du style ---- Maxéville, 2e lot 1903.
Nombre des individus.
Millimètres.

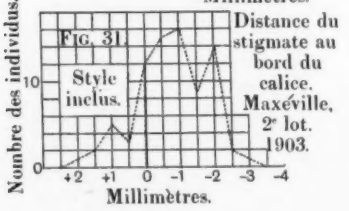


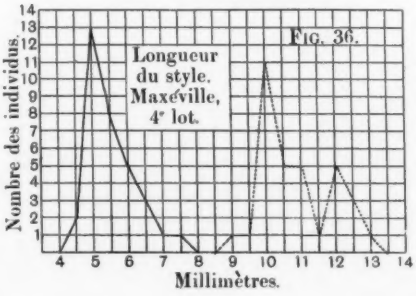




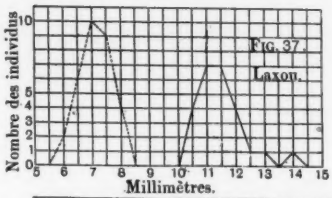


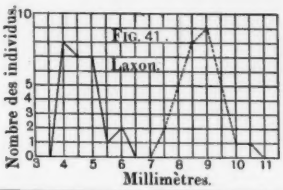


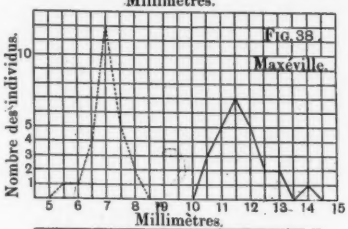


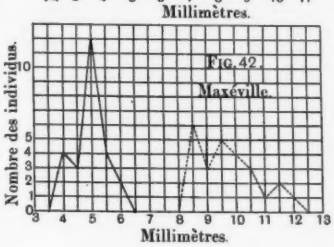


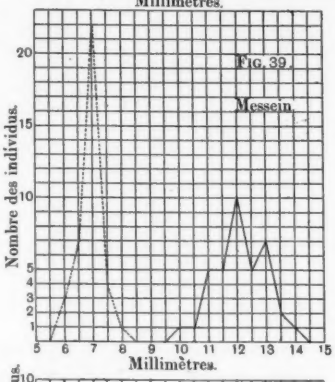
Brévistylées —
Brévistémonées ----
Voyez Tabl. XII.

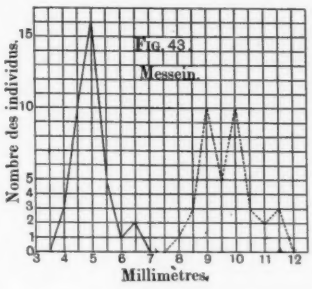


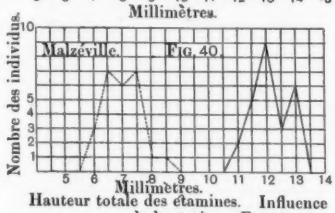




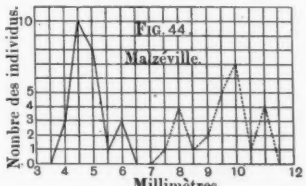




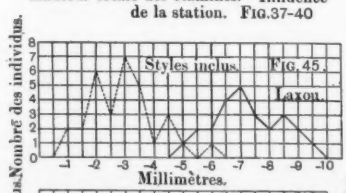


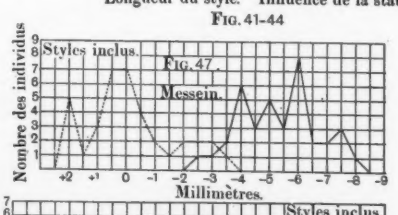


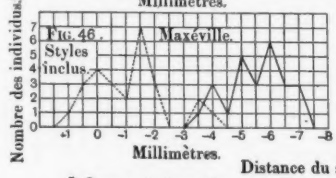
Hauteur totale des étamines. Influence de la station. FIG. 37-40

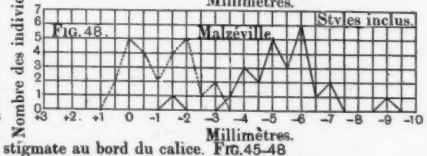


Longueur du style. Influence de la station. FIG. 41-44









Distance du stigmate au bord du calice. FIG. 45-48

Influence de la station (voir Tableaux XV—XVIII). Plantes brévistémonées - - - - - -
Plantes brévistylées ——

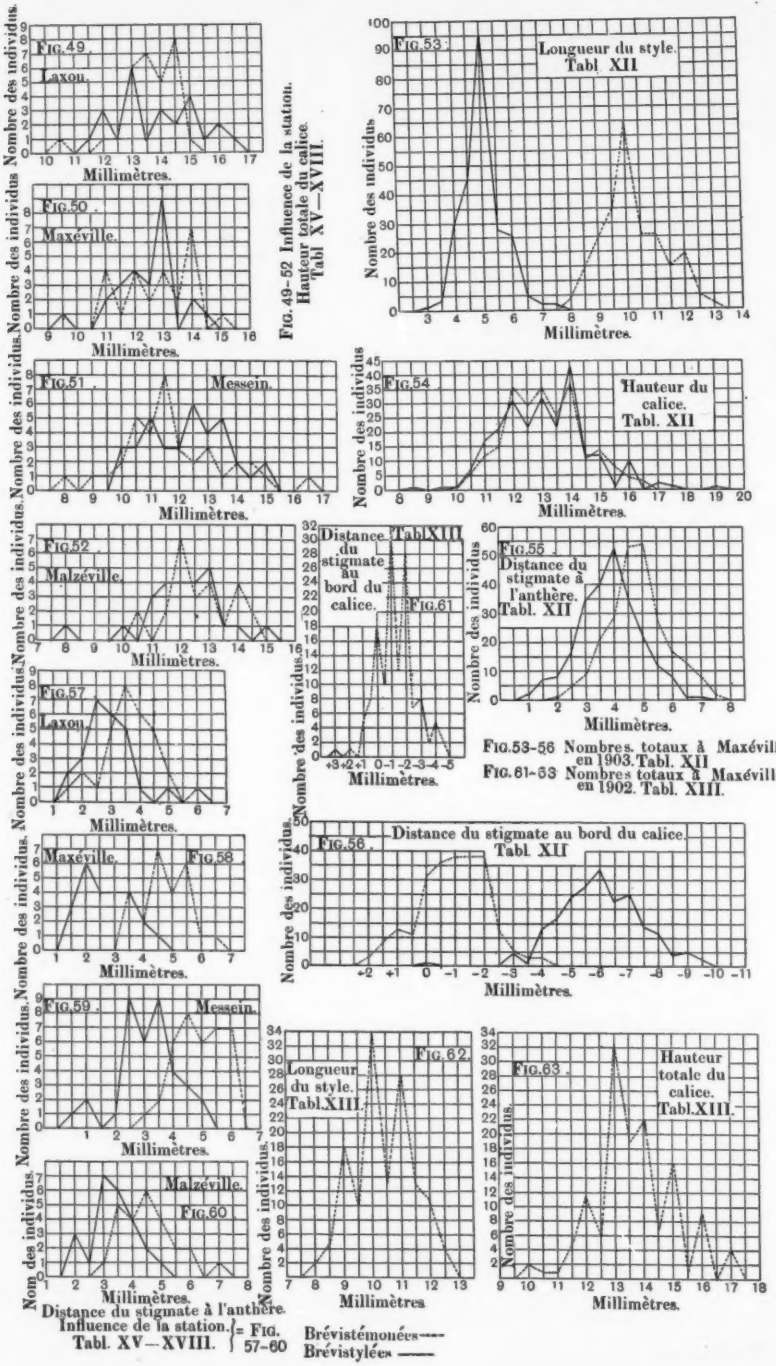


Fig. 49. Laxou.
Fig. 50. Maxéville.
Fig. 51. Messein.
Fig. 52. Malzéville.
Fig. 57. Laxou.
Maxéville. Fig. 58.
Fig. 59. Messein.
Malzéville. Fig. 60.
Nombre des individus.
Millimètres.
Distance du stigmate à l'anthère.
Influence de la station. Tabl. XV—XVIII. = Fig. 57-60
Fig. 49-52 Influence de la station. Hauteur totale du calice. Tabl. XV—XVIII.
Fig. 53. Longueur du style. Tabl. XII
Fig. 54. Hauteur du calice. Tabl. XII
Distance du stigmate au bord du calice. Tabl XIII Fig. 61
Fig. 55. Distance du stigmate à l'anthère. Tabl. XII
Fig. 53-56 Nombres totaux à Maxéville en 1903. Tabl. XII
Fig. 61-63 Nombres totaux à Maxéville en 1902. Tabl. XIII.
Fig. 56. Distance du stigmate au bord du calice. Tabl. XII
Fig. 62. Longueur du style. Tabl. XIII.
Fig. 63. Hauteur totale du calice. Tabl. XIII.
Brévistémonées ----
Brévistylées ——

MISCELLANEA.

I. On the Correlation between Hair Colour and Eye Colour in Man.

It seems desirable in view of the recent discussions on the relationship between coat pigment and eye colour in certain mammals to ascertain whether there exists any constant relationship between these characters in the case of man. I have been able to collect the following tabled material, wherein far greater numbers have been reduced to permilles.

I. *Swedish Returns due to G. Retzius.*

Hair Colour.

Eye Colour.	Blond	Braun	Schwarz	Roth	Totals
Hell ...	544	105	2	16	667
Melist ...	191	87	4	6	288
Braun ...	18	24	2	1	45
Totals ...	753	216	8	23	1000

II. *Prussian Returns due to R. Virchow.*

Hair Colour.

Eye Colour.	Blond	Braun	Schwarz	Brandroth	Totals
Blau ...	355	73	0	1	429
Grau ...	242	78	5	1	326
Braun ...	127	109	8	1	245
Totals ...	724	260	13	3	1000

III. *Italian Returns due to R. Livi.*

Hair Colour.

Eye Colour.	Blond	Braun	Schwarz	Roth	Totals
Hell ...	30	60	12	1	103
Melist ...	27	132	45	2	206
Braun ...	25	409	254	3	691
Totals ...	82	601	311	6	1000

IV. *Germano-Jewish Returns due to R. Virchow.*

Hair Colour.

Eye Colour.	Blond	Braun	Schwarz	Brandroth	Totals
Blau ...	114	82	0	1	197
Grau ...	113	137	25	1	276
Braun ...	100	333	92	2	527
Totals ...	327	552	117	4	1000

V. *Baden Returns due to O. Ammon.*

Hair Colour.

Eye Colour.	Blond	Braun	Schwarz	Roth	Totals
Hell ...	353	209	72	10	644
Melist ...	46	112	66	5	229
Braun ...	17	65	43	2	127
Totals ...	416	386	181	17	1000

I have quoted the permille results in Tables I, III and V direct from Retzius and Fürst's *Anthropologia Suecica*, p. 155, and preserved their German terminology for describing hair and eye colours so that I may mark the fact that they consider the German classes to cover also the Italian-Swedish groupings. The original source of the Italian returns is R. Livi: *Antropometria militare*, Roma, 1898, and of the Baden, Otto Ammon: *Zur Anthropologie der Badenser*, 1899. Those for Prussian and Jewish children I have taken from the paper by Rudolf Virchow: *Farbe der Haare und der Augen der Schulkinder*, *Archiv für Anthropologie*, Bd. XVI. 1886, S. 468, etc. In II 4,127,744 children were classified, and in IV, 74,146. The correlations were determined from these numbers before reducing to permilles.

VI. *British Returns due to K. Pearson.*

Hair Colour.

Eye Colour.	Fair	Brown	Dark	Red	Totals
Light ...	210	107	47	20	384
Medium ...	117	158	113	15	403
Dark ...	23	63	125	2	213
Totals ...	350	328	285	37	1000

My data are from my observations on school children, in this case for boys only. I have put my categories 'Dark' and 'Jet Black' together as 'Dark' as the nearest equivalent to the continental 'Schwarz' possible under the circumstances. My three eye categories cannot be very different from the 'Hell,' 'Melist' and 'Braun' of Retzius' tables. Their exacter definitions will be found in *Biometrika*, Vol. III. p. 162. The importance of complete equivalence is, however, for our present purposes, not very great. We want to get the same number of reasonably definite groupings in each case. The six tables above each supply a 12-fold classification. From these tables the correlation between hair and eye colours was determined for me by Dr A. Lee using the method of mean square contingency,—a method which gives results independent of any scale order whatever*. We found

Correlation between Hair and Eye Colours.

Sweden	(Conscripts)	·2495
Prussia	(Schoolboys and Schoolgirls)	·2714
Italy	(Conscripts)	·3091
German Jews	(Schoolboys and Schoolgirls)	·3381
Baden	(Conscripts)	·3540
Great Britain	(Schoolboys)	·4203

Now these results seem to show that the correlation between hair and eye colours is by no means so close as has been hitherto supposed. Further it appears to be less in those districts which, whether light or dark, have a majority of one type. It is not improbable that the higher values in Baden and Great Britain are due to the effect of a greater mixture of local races, such local races not having interbred largely. In my opinion the results for the German Jews show that the population so classified is very far from being purely Semitic. If as I suspect the lesser values of the correlation occur in the *relatively* more homogeneous groups, then we might infer that increasing purity of race would mark a still slighter correlation between hair and eye pigment. In other words, if mankind originally consisted of several races each with a definite pure eye and pure hair colour, then the right statement is that the correlation between hair and eye colours was *interracially* unity or perfect, and *intraracially* zero. The point is of course one of terminology, provided the decreasing intraracial correlation with increasing racial purity be once established as a fact. But it is an important point of terminology, especially when pure races are crossed for hybridisation experiments. Is it correct to say that the correlation in white mice between eye pigment and coat colour is perfect? Since both characters are invariable, algebraically the correlation takes the indeterminate form 0/0. It is not unusual

* Pearson: "Mathematical Contributions to the Theory of Evolution. XIII. On the Theory of Contingency and its Relation to Association and Normal Correlation." *Drapers' Research Memoirs, Biometric Series, I.* (Dulau and Co., Soho Square, London.)

to call it 'perfect,' but I think the statement must be reconsidered in the light of the suggestion made in this note that possibly increasing racial purity marks decreasing correlation. The 'perfect' correlation in the case of the white mice is really a sub-conscious transfer of the idea, based on experience of white and grey mice, of an *interracial* correlation coefficient equal to unity. It is one of the many points where caution is needful in passing from the old conception of correlation in the sense of Cuvier as an association of two attributes to the modern biometric notion as a relation between deviations.

K. P.

II. On the Correlation between Age and the Colour of Hair and Eyes in Man*.

In recent work the resemblance of siblings in hair and eye colour has been used for measuring the intensity of collateral inheritance. In determining the correlation in pigmentation between siblings, allowance ought to be made for change of pigmentation with age supposing as in the case of man, we do not, as with greyhound puppies or thoroughbred yearlings, take the siblings at sensibly the same ages†. It is usually stated that eye and especially hair colour are modified by age, but hitherto no quantitative measure of the change seems to have been published. It is the object of the present paper to determine measures of this kind. Prussian school children as given by Rudolf Virchow ("Gesammtbericht über die von der deutschen anthropologischen Gesellschaft veranlassten Erhebungen über die Farbe der Haut, der Haare und der Augen der Schulkinder in Deutschland," *Archiv für Anthropologie*, Bd. XVI. 1886, S. 468–9) provided the following data. See Tables I and II. In these tables the so-called "andere Combinationen" were omitted as no ages were given; they are, however, very insignificant in total number. Using the method of mean square contingency, we have for children between 6 and 12 years of age:

Correlation of Age and Hair pigmentation = ·033.

Correlation of Age and Eye pigmentation = ·027.

TABLE I.

Age and Hair Colour, Prussian School Children.

Hair Colour.

Haare	Blonde	Braune	Schwarze	Brandrothe	Totals
Age *6— 8*	409,830	129,225	5,172	1,622	545,849
8—10	1,565,036	555,287	25,736	5,405	2,151,464
10—12	488,938	190,729	8,866	2,208	690,741
12—14	131,883	55,023	2,443	602	189,951
Totals	2,595,687	930,264	42,217	9,837	3,578,005

* I have put together the following results from Notes on this subject made by Dr Ginzo Uchida when working in my laboratory and placed by him at my disposal. K. P.

† The allowance is easy to make, if the correlation between pigmentation and age is known. See Pearson: *R. S. Proc.* Vol. 71, pp. 289—294.

TABLE II.

Age and Eye Colour, Prussian School Children.

Eye Colour.

Augen	Blaue	Graue	Braune	Totals
Age *6— 8*	227,223	178,014	140,612	545,849
8—10	946,576	692,249	512,639	2,151,464
10—12	292,756	229,221	168,764	690,741
12—14	74,277	66,445	49,229	189,951
Totals	1,540,832	1,165,929	871,244	3,578,005

The children above 14 have been omitted because few in number they come from a selected class, while the above compose the whole child population in the elementary schools.

It will be seen at once that the correlations are far lower than might have been anticipated. Indeed so low as to make no change of significance when we are dealing with the correlation of hair and eye colour in children, or with the degree of resemblance between siblings of 6 to 14 years of age*.

Virchow's data, however, combine the two sexes and it seemed desirable to consider the point for one sex only and possibly for a greater range of ages. Accordingly the following Tables were prepared from a portion of the sister-sister series of school data papers collected by Prof. Pearson.

TABLE III.

Age and Hair Colour, British Schoolgirls.

Hair Colour.

	Red	Fair	Brown	Dark	Jet Black	Totals
Age *4— 7*	2	16	6	3	0	27
7—10	10	86	79	20	0	195
10—13	16	165	160·5	73·5	4	419
13—16	19	136·5	189·5	91	3	439
16—19	6·5	71·5	90·5	55	1·5	225
Totals	53·5	475	525·5	242·5	8·5	1305

* Pearson, Huxley Lecture. See *Biometrika*, Vol. III. pp. 149—150.

TABLE IV.

Age and Eye Colour, British Schoolgirls.

Eye Colour*.

	Light	Medium	Dark	Totals
Age *7—10*	73	74	41	192
10—13	147	180	85	412
13—16	120	214	96	430
16—19	64	99	57	220
Totals	404	571	279	1254

From these Tables by the method of mean square contingency we have:

Correlation between Age and Hair Colour = ·158.

Correlation between Age and Eye Colour = ·096.

Thus by dealing with one sex only and going on to 19 years of age, we see that the correlations while still not large are yet sensibly increased. If we may compare the German and English material, we should say that there is little change in hair or eye colour with children under 14; by this we mean, not enough to influence the determination of pigmentation resemblance of children of different ages. But after 14 there is even before 19 a more marked change, the correlations still, however, remaining low. This change is much more considerable in the case of hair than of eye colour, though sensible in both. Even thus the values of the collateral heredity coefficients, if we allowed for change of age, would hardly be influenced within the limits of their probable errors.

It would be interesting to trace the change in the correlation for one sex and race for later ages, but the material does not appear to be available. There is a table "showing the colour of eyes and hair of both sexes at all ages of English and Welsh origin" in the Final Report of the Anthropometric Committee of the British Association (1883)†. But the classification is different from that used in our data. Moreover the results are classified first according to eye colour, and in the case of eyes of a 'neutral' colour, the hair colour does not appear to be distinguished. Thus it is impossible to base a correlation table of hair colour at different ages on the B.A. data, and no satisfactory comparison can be made with our present results.

The only other material which is known to us is embodied in the *Sozialanthropologische Studien* of the late Dr W. Pfitzner of Strasburg, whose researches are in many respects both valuable and suggestive. But even where he deals with fairly numerous data‡, certain limitations soon appear. Thus for the period from 5 to 20 years he examined only 164 girls, and it seems impossible to determine a correlation from such numbers. Moreover his figures are exclusively based on observations made on the corpse. He ought therefore to have ascertained as a pre-

* Dr Uchida has omitted the ages 4 to 7 as of very small frequency. Tables for much larger numbers will be eventually published. Dr Uchida has not included the 'blue' schedules, principally infant schools, at all. Material is probably available for nearly 2,500 girls. K. P.

† *Report of the British Association*, 1883, pp. 278–9.

‡ Schwalbe's *Zeitschrift für Morphologie*, Bd. I. S. 372.

liminary whether the proportions of different hair colours in the case of those who die early and in hospitals are the same as those of healthy children in the population at large. On the basis of our statistics this is improbable. A selective death rate in the matter of pigmentation, and peculiar selection of subjects, vitiates largely, we believe, returns based upon post-mortem room observations. All that has been done with Pfitzner's observations was to work out the Table for males from 0 to 75 years of age. This is given below.

TABLE V.

Correlation of Age and Hair Colour. Males, Lower Elsass.

	Hair Colour.			
	Blond	Braun	Schwarz	Totals
Age *0—15*	384 {83·8}	74 {16·2}	0 { 0}	458
15—30	122 {35·8}	204 {59·8}	15 { 4·4}	341
30—45	119 {28·0}	261 {61·6}	44 {10·4}	424
45—60	120 {24·6}	292 {60·6}	75 {15·4}	487
60—75	58 {28·7}	105 {52·0}	39 {19·3}	202
Totals	803	936	173	1912

Using the method of mean square contingency we find:

Correlation between Age and Hair Colour = ·451.

Now if we were to trust this table we should have a very marked increase in black hair with increasing age, and this is the interpretation which Dr Pfitzner put upon results like this in his memoir. But a little consideration shows that this cannot be correct. Are we to say that there are no children with black hair in Lower Elsass, and that the 20 per cent. found among old persons is due to darkened pigmentation only? Prussia is lighter than Elsass but it contains 18 per 1000 of such children. We should have expected at least eight or nine black-haired children in Pfitzner's 458 under 15 years of age. Baden close to Elsass shows 18 per cent. of conscripts with black hair, while Pfitzner gives only 4·4 with black hair between 15 and 30! Pfitzner's results, if attributed to age-effect, seem quite incompatible with what is known of the normal population for Elsass. They are, however, quite comprehensible if there be a positive correlation between fairness and disease in childhood. Now this is exactly what our British school children show: *there is a correlation between health and darkness of hair colour.* Hence if we do not follow up individuals, noting their pigmentation at different ages, but simply correlate age of different individuals with hair colour, we are liable to exaggerate the correlation between age and pigmentation, and this will be especially the case, if we use hospital returns. Hence, it is probable that our neglect of a selective death rate, based upon the known correlation between general health and pigmentation, really emphasises the values found for correlation between pigmentation and age. Further, while it is probable that if we take adult life into account we should find this correlation increased—perhaps even to ·2 or ·25—the value deduced from Pfitzner's observations of ·45 may be safely considered to mark in the first place a selective death rate, i.e. a correlation between fitness *in childhood* and dark pigmentation.

In conclusion it may not be without interest to exhibit the results for age, hair and eye colours of British Schoolgirls from 7 to 19 in a single table :

TABLE VI.

Correlation of Age with Hair and Eye Colours.

Hair Colour.

	Eyes	Red	Fair	Brown	Dark	Totals	
Age *7—10*	Light	4 {5·5}	52 {71·2}	17 {23·3}	0 { 0 }	73	192
	Medium	5 {6·4}	27 {34·6}	40 {51·3}	6 { 7·7}	78	
	Dark	1 {2·4}	6 {14·6}	20 {48·8}	14 {34·2}	41	
10—13	Light	6 {4·1}	92·5 {63·1}	43·5 {29·4}	5 { 3·4}	147	412
	Medium	10 {5·6}	57·5 {31·9}	82 {45·6}	30·5 {16·9}	180	
	Dark	0 {0 }	12 {14·1}	31 {36·5}	42 {47·1}	85	
13—16	Light	4 {3·3}	62 {51·7}	45·5 {37·9}	8·5 { 7·1}	120	430
	Medium	8·5 {4·0}	66·5 {31·1}	103 {48·1}	36 {16·8}	214	
	Dark	5 {5·2}	5·5 { 5·7}	38 {39·6}	47·5 {49·5}	96	
16—19	Light	3 {4·7}	31 {48·4}	21·5 {33·6}	8·5 {13·3}	64	220
	Medium	2 {2·0}	34 {34·3}	48 {48·5}	15 {15·2}	99	
	Dark	1·5 {2·6}	45 { 7·9}	18 {31·6}	33 {57·9}	57	
Totals	—	50 {4·2}	450·5 {58·9}	507·5 {31·5}	246 { 5·4}	1254	
All Ages	Light	17 {4·2}	237·5 {58·9}	127·5 {31·5}	22 { 5·4}	404	
	Medium	25·5 {4·5}	185 {32·4}	273 {47·8}	87·5 {15·3}	571	
	Dark	7·5 {2·7}	28 {10·0}	107 {38·4}	136·5 {48·9}	279	

The numbers in curled brackets give the percentages of each hair colour of girls of a given age having a given eye colour. Examining this table it would seem doubtful, having regard to the paucity of individuals dealt with, whether we can assert significant changes in the percentages of medium-eyed girls having fair or brown hair at different ages. Nor would it be wise to insist that the changes of percentages in red-haired girls with light or dark eyes are significant. Red-haired girls with medium eyes seem to become continuously fewer with age; light-eyed girls with fair hair become significantly fewer, and brown-haired girls with light eyes more numerous. Dark-eyed girls with fair or brown hair become significantly fewer and dark-eyed girls with dark hair become more numerous, and probably light-eyed girls with dark hair also. The medium-eyed girls with dark hair (except in infancy?) remain much the same in percentage. Thus except in the case of red-haired girls those with medium eye colour change least; the fair-haired girls with light eyes tend to become brown or even dark, and the dark-eyed girls with fair or brown hair to become dark-haired. How far these changes are influenced by a relative death rate still remains to be determined.

III. On the Contingency between Occupation in the Case of Fathers and Sons.

By EMILY PERRIN.

It is a problem of some interest to determine how far ancestral bent and how far environmental conditions influence a man in his choice of occupation in life. The discussion of this problem has become feasible since the introduction of the new method of contingency into the statistical treatment of related variables*. By this method all questions of continuity and of scale in the variables are dispensed with. We fall back on the simplest and most fundamental of ideas—a measurement of the deviation from independent probability in the case of the two variable characters. The coefficient of contingency measures the degree of association or dependence between any two series and becomes more and more nearly the coefficient of correlation as the material approaches normality. In Professor Pearson's memoir on Contingency he deals in Illustration D† with my first statistics of occupational contingency. These covered 775 cases of occupation for father and son classified into 14 groups; the material was extracted from the *Dictionary of National Biography*. I have since doubled the number of extracted cases, and taken an additional 1550 from *Who's Who*. I have kept the two materials quite distinct for comparative purposes. There are undoubtedly great differences in their character, although both sources of record are of course subject to selection, i.e. either father or son must have reached a moderate amount of distinction to be entered in either work. The following Tables give my material.

TABLE I.

Contingency between Occupations of Fathers and Sons (from Who's Who).

Occupation of Son.

Occupation of Father.	Army	Art	Teacher, Clerk, Civil Servant	Crafts	Divinity	Agriculture	Landownership	Law	Literature	Commerce	Medicine	Navy	Politics and Court	Scholarship and Science	Totals
Army	148	4	20	—	4	—	3	10	12	2	2	9	9	6	229
Art	—	38	2	—	2	—	1	1	7	—	3	1	—	8	63
Teacher, Clerk, Civil Servant	13	4	36	—	11	—	—	5	10	5	2	3	2	5	96
Crafts... ...	—	2	—	1	1	—	—	—	—	—	—	—	—	—	4
Divinity ...	58	13	52	—	80	—	5	42	53	5	15	10	16	62	411
Agriculture ...	—	—	1	—	2	1	—	—	3	—	—	—	2	1	10
Landownership	42	5	17	—	15	—	20	24	13	10	10	15	30	14	215
Law	9	5	13	—	11	—	—	40	16	1	12	1	7	8	123
Literature ...	1	5	1	—	2	—	—	4	17	—	1	—	—	2	33
Commerce ...	4	8	6	—	11	—	—	19	12	33	3	1	2	17	116
Medicine ...	15	11	11	—	8	—	9	9	3	1	33	1	3	5	109
Navy	25	1	4	—	5	—	3	3	3	1	4	16	3	2	70
Politics & Court	3	—	3	—	—	—	—	1	3	1	—	—	15	—	26
Scholarship and Science	2	1	2	—	—	—	1	5	3	1	1	—	1	28	45
Totals	320	97	168	1	152	1	42	163	155	60	86	57	90	158	1550

* "Mathematical Contributions to the Theory of Evolution. XIII. On the Theory of Contingency and its Relation to Association and Normal Correlation." By Karl Pearson. (Dulau & Co., London, 1904.)

† *Loc. cit.* p. 32 *et seq.*

TABLE II.

Contingency between Occupations of Fathers and Sons (from Dictionary of National Biography).

Occupation of Son.

Occupation of Father.	Army	Art	Teacher, Clerk, Civil Servant	Crafts	Divinity	Agriculture	Landownership	Law	Literature	Commerce	Medicine	Navy	Politics and Court	Scholarship and Science	Totals
Army	49	—	8	—	5	—	2	6	5	—	4	3	9	4	95
Art	2	105	1	1	3	—	—	2	4	—	—	1	1	2	122
Teacher, Clerk, Civil Servant	8	12	15	1	15	1	3	10	8	2	4	2	4	12	97
Crafts	1	21	1	14	7	—	—	2	14	5	3	—	—	18	86
Divinity ...	13	5	6	—	121	—	1	17	21	5	20	8	7	26	250
Agriculture ...	2	4	4	4	6	2	—	1	17	3	5	2	1	15	66
Landownership	23	3	7	—	14	8	8	22	9	1	10	12	37	15	169
Law	6	8	10	—	10	—	2	53	23	4	6	3	9	13	147
Literature ...	1	1	1	—	5	—	—	4	11	1	2	1	2	4	33
Commerce ...	16	31	9	2	35	—	—	16	31	39	9	3	12	38	241
Medicine ...	2	7	5	—	5	1	—	1	5	—	29	—	6	13	74
Navy	1	3	2	—	1	—	2	2	3	1	2	13	3	2	35
Politics & Court	13	—	3	—	4	—	2	9	3	—	2	3	34	1	74
Scholarship and Science	9	3	1	2	8	—	1	7	3	3	2	1	1	20	61
Totals	146	203	73	24	239	12	21	152	157	64	98	52	126	183	1550

It will be seen at once how the temporary official class which largely fills *Who's Who*, crowding Army, Navy and Civil Service columns, finds a much smaller place in the record of national importance. On the other hand Literature represented by 155 fathers and 33 sons in *Who's Who* contributes 157 fathers and 33 sons to a random sample of the same number from the *Dictionary*. The relative distinction of Literature is thus the same in the present and the past. Other single entries will be found on comparison of some suggestiveness, but I do not stay over them here.

Using Professor Pearson's notation I find:

For *Who's Who* (1550 cases):

Mean Square Contingency, $\phi^2 = 1{\cdot}285{,}868$,
Coefficient of Contingency, $C_1 = {\cdot}7500$.

For *Dictionary of National Biography* (1550 cases):

Mean Square Contingency, $\phi^2 = 1{\cdot}413{,}601$,
Coefficient of Contingency, $C_1 = {\cdot}7653$.

Professor Pearson for my first Table of 775 entries from the *Dictionary* gives*

Mean Square Contingency, $\phi^2 = 1{\cdot}299{,}206$,
Coefficient of Contingency, $C_1 = {\cdot}6275$.

* *Loc. cit.* p. 34.

But $C_1 = \sqrt{\phi^2/(1+\phi^2)}$, and thus in the last case we ought to have $C_1 = \cdot 7517$, and not $\cdot 6275$, which is due to an oversight in the arithmetic.

Thus we see that the coefficient of contingency whether found from a sample of 775 or one of 1550 from the *Dictionary* has sensibly the same value, and this value is identical with one found with the same classification and the same number of cases from such an entirely different source as the annual *Who's Who*. It seems clear that whether we take the present, or the long period of the past embraced by the *Dictionary*, the environmental influences which induce a man in this country to follow his father's occupation must have remained very steady. The coefficient of contingency for parental inheritance will be like the coefficient of correlation about $\cdot 5$. I think, therefore, we may say that in the choice of a profession inherited taste counts for about $\frac{2}{3}$ and environmental conditions for about $\frac{1}{3}$. These numbers of 2 to 1 are somewhat less than the 3 to 1 given by Professor Pearson on the basis of the erroneous value $\cdot 6275$ cited above. It would be extremely interesting to compare these results for an old country like Great Britain with those for a new country like America. *A priori* we should expect to find a greater freedom from environmental influences, a greater choice in the son, and so a nearer approach to a pure inheritance of taste.

IV. On a Convenient Means of Drawing Curves to Various Scales.

By G. UDNY YULE, Newmarch Lecturer in Statistics, University College, London.

Let an ordinary scale of equal parts, say inches, be engraved on the moving blade of a "clinograph," or adjustable set square, the zero point of the scale being at the lower end. If the blade be set at an angle θ to the horizontal, the vertical distances of the points 1, 2, 3... of the scale above a horizontal line XX drawn through the zero point, are evidently $\sin\theta$, $2\sin\theta$, $3\sin\theta$, etc. Hence if a curve be plotted to the base XX, with the scale maintained at this inclination, it will be drawn with a scale of which the unit is $\sin\theta$, where θ may take any value we please from 0° to 90°. The plotting proceeds in the ordinary straightforward way. Supposing two ordinates to be plotted are 8·95, 7·63, the clinograph is slipped along the T-square until the 8·95 of the scale falls over the proper vertical, when the point is pricked off; the clinograph is again shifted till the 7·63 of the scale comes over the second vertical and its value is similarly marked, and so on. A scale of variable inclination thus becomes, for plotting purposes, a scale with a continuously variable unit.

Such a scale is particularly convenient for plotting certain curves of given equation, e.g. the normal curves of errors,

$$y = y_0 e^{-\frac{x^2}{2\sigma^2}},$$

y_0 being the ordinate at the mean, and σ the standard deviation. In the ordinary way, the curve is plotted by the aid of tables giving the value of $e^{-\frac{1}{2}t^2}$, the most complete tables being those of Mr W. F. Sheppard (*Biometrika*, Vol. II. pp. 174—190). Intervals of, say, $\frac{1}{5}$th of the standard deviation are marked off along the base in either direction from the mean, ordinates erected at these points, and their magnitudes plotted from the tabular values multiplied by y_0. With the inclined scale this process may be considerably abbreviated.

To divide the base, the T-square or straight-edge is turned round at right angles* to the base, a length, say MS, equal to the standard deviation is plotted from the mean along XX, and the

* If the adjustment be made as described below, any inclination will do. Hence it is of no consequence if the two edges of the drawing board are not at right-angles.

inclination of the scale is adjusted so that, when it moves along the T-square, the zero-point will pass over the mean M and the point 5 over the point S. The units of the scale will then give fifths of the standard deviation. Once the inclination has been adjusted, the T-square may be moved up or down if necessary to complete the plotting. The verticals are then drawn in, and the value of y_0 marked off on the central ordinate. The T-square being now placed parallel to the base, the scale and square are adjusted so that the zero point lies over the base and the point 10 over the top of y_0. The tabular values of $e^{-\frac{1}{2}s^2}$ then give the remaining ordinates directly from the scale readings.

Even the trouble of extracting or reading the tabular values may however be eliminated, if a special scale be engraved, in which the distances of the divisions from the left-extremity are proportional to the ordinates $e^{-\frac{1}{2}s^2}$. The inclination of the moving blade being once adjusted as above, the remaining ordinates are plotted direct from the divisions of the scale without interpolation.

As a matter of practice I have thought it best for the scales to be made, as in fig. 2, with the joint like an ordinary carpenter's rule, such a joint being stiff and wearing fairly well. The centre portion of each edge of the rule is bevelled for plotting, but the ends are left thick to prevent the rule slipping under the T-square or straight-edge. Four scales can be engraved on such a rule.

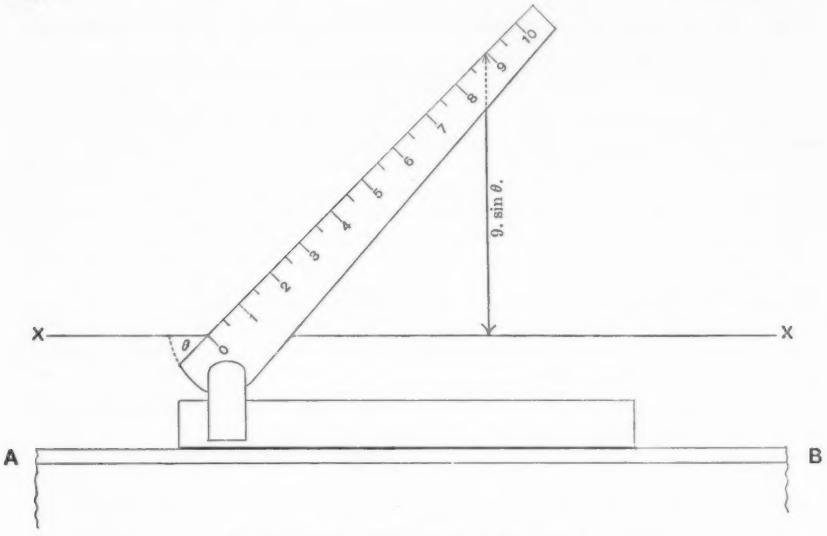


FIG. 1. AB is a straight-edge or T-square.

It is evident that the principle is applicable to a variety of curves, and I have had special scales engraved for plotting ellipses (the scale being given by equidistant ordinates in a quadrant of a circle), arcs of parabolas (such as are required in drawing parabolic girders, bending moment curves, etc.), curves of sines or cosines, and the normal curve of errors. The scales were made for me by A. G. Thornton, King Street West, Manchester. For statistical work, three plain scales of different units and divisions and a normal-curve scale form a convenient set. With the plain scales, curves of any sort are readily plotted with any desired ratio of length to breadth, e.g. for reproduction on a given page or lantern slide.

The idea of using a scale at a variable inclination is such a simple one, that it scarcely seems possible it can be novel, though new to me. Many ideas of considerable antiquity, e.g. "Gunter's Lines," come extremely close to it. The advantage, however, of the inclined scale, used direct on the drawing board, over the Gunter's lines (scales divided on lines passing through the pivot of the carpenter's rule), is very considerable in point of time, the use of dividers being avoided altogether.

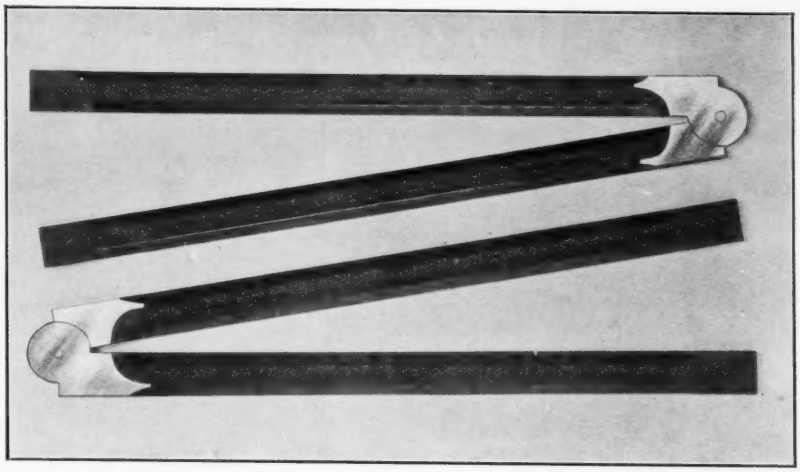


Fig. 2.

V. Albinism in Sicily—A Correction.

The abstract of *Biometrika*, Vol. III. Pt. I. contains this passage: "W. F. R. Weldon shows that the data from Sicily provided by Arcoleo are not in accordance with Mendelianism nor with the theory of gametic purity." These words fairly give the purport of the article appearing at p. 107 of that number, over Professor Weldon's initials.

On looking into the matter I find that the more serious of the two considerations Professor Weldon adduces, namely, the production of two pigmented children as well as three albinos by two *albino* parents, rests on a mistake. His acquaintance with Arcoleo's work is derived from *Archivio per l' Antropologia*, I. 1871, but he apparently failed to notice that this was not an original communication. The original, from which the *Archivio* copied a part only, appears in *Gazeta Clinica* dello Spedale Civico di Palermo, F. II. (1871). Here Arcoleo states explicitly that he never met with any case of an albino child being born to an albino parent. The following passages give his words*.

"Passando all' albinismo della specie umana, si chiede: è anch' esso ereditario? Per dirsi ereditario un vizio o un morbo, bisogna che si ripeta nei discendenti nel modo onde esiste negli ascendenti. Eppure in 24 famiglie da me esaminate, in nessuna è preesistito un esempio di tal genere, nè nei 24 figli procreati dai cinque albini coniugati ve ne ha uno solo che richiami il tipo albino del padre o della madre. Parrebbe adunque che l' albinismo nella specie umana non fosse affatto ereditario, &c."

* Pp. 12—13 of reprint. I cannot give the original pagination.

Again: "Non essendo ancora dimostrato l' albinismo ereditario nell' uomo, si dimanderebbe sempre; quale è la causa prossima che lo ingenera?"

Therefore the parents of family No. 7 were not as Professor Weldon states, both albinos, but *both pigmented*. The words which misled him, "Gli antenati furono tutti bianchissimi" plainly refer to "the extraordinary fairness of many blonde persons, who were still slightly pigmented," a phenomenon witnessed by Professor Weldon himself in Sicily.

On the other hand this correction somewhat increases the discrepancy from Mendelian expectation to which Professor Weldon also calls attention. The families from supposed *DR* parents, as corrected, contained 86 pigmented, and 52 albinos, the expectation being 103·5 and 34·5, a notable deficiency of normals.

For several reasons I suspect that these numbers must be taxed before any deduction can be drawn with great confidence from the deficiency of normals; but it is by no means improbable that the deficiency does indicate some real complication, which indeed may on other grounds be already apprehended.

When I had occasion to refer to albinism in Man I wrote as follows (*P. Z. S.*, 1903, II. p. 77) "Naturally we may inquire whether albinism in Man is not a similar recessive. Castle has given evidence pointing in this direction. The occurrence of albinism in the families of first cousins (see Day, Seligmann, &c.) is consistent with this view; but there are a few recorded cases of the occurrence of albinos in the offspring of albinos breeding with normal parents, where the hypothesis that the normal parent was *DR* is not at all easily admissible. No case of the union of two human albinos is known to me. The matter cannot here be further discussed, and the reader must refer to the literature, the most important paper being that of Cornaz."

These words seem to me to express very clearly the doubt I then felt (1903). It was surprising to find them transformed by Professor Weldon into the statement that "the suggestion [that albinism in Man has a Mendelian behaviour] is considered probable by Bateson." Nevertheless after further experience, and especially after study of Arcoleo's paper (previously known to me by title only) with its record of 5 families bred within the 2nd canonical degree of relationship, I now lean with some decision towards the view that albinism in Man will be shown to have a Mendelian inheritance, possibly, as I said above, with a complication.

W. BATESON.

25 *November*, 1904.

[I think Dr Arcoleo's memoir hardly bears out in one respect the interpretation Mr Bateson puts upon it. I am not clear what Dr Arcoleo means by the words cited by Mr Bateson at the bottom of p. 471. The emphasis may be on *nel modo* and *di tal genere*. They may refer to the special type and intensity of the albinism, for example in regard to defective vision and nystagmus. But I do not think it possible to interpret them in the sense of Mr Bateson: "Here Arcoleo states explicitly that he never met with any case of an albino child being born to an albino parent." Dr Arcoleo states explicitly that the mother of the four albinos in Family No. 6 was "una albina di belle forme," and the union (within the second canonical degree) arose from the desire on the part of the Cav. N. N. to have an albino daughter like her. If the *Cavaliere* was *DD* then the case is in favour of Professor Weldon; if he was *DR*, then the result in this family, 4 albinotic and 7 pigmented offspring, was reasonably in accordance with the Mendelian expectation, which is $5\,5 \pm 1$. K. P.]

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